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CONTENTS.

Articles.	PAGE
1. Reichenbach and the Psychical Research Society	313
2. Atlantis Once More	319
3. A New Theory concerning the Burning Mountains. By A. H. Swinton	327
4. Wealth as an Eliminative Agent. By W. Mattieu Williams	332
5. The Action of Poisons on the Petals of Flowers. By A. A. Nesbit, F.C.S.	334
6. Haeckel, Huxley, and Mivart on the Scope and Provinces of Zoology. By Oswald Dawson	336
7. Notes on the Mias. By W. de Crespigny, of Sarawak	347
Analyses of Books.	
Physiological Cruelty, or Fact *v.* Fancy: an Enquiry into the Vivisection Question	350
Light Science for Leisure Hours	355
Longman's Magazine	358
The Mason College Magazine	360
The Popular Science Monthly	361
Sound	363
Correspondence.	
Do Snakes Shelter their Young in their Throats? By C. C. Hopley &c., &c.	365
Notes	370

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THE

JOURNAL OF SCIENCE.

JUNE, 1883.

I. REICHENBACH AND THE PSYCHICAL RESEARCH SOCIETY.

NEARLY forty years ago the scientific world and the more intelligent classes in general were startled by an announcement in Liebig's "Annalen." Baron von Reichenbach, who, though not a chemist of the highest order, was still favourably known as an experimentalist, had observed luminous appearances over the poles of powerful magnets, at the ends of the axes of certain crystals, and upon a number of other bodies. These phenomena Reichenbach ascribed to a yet undiscovered form of energy, to which he gave the name of Od. At once not a few enquirers attempted to verify the author's results, but in general with but indifferent success. The misfortune was that, on Reichenbach's own admission, these lights were not visible to all mankind, but only to a minority, whose sight was abnormally delicate. The very name which had been selected was, for England at least, very unfortunate. The majority of our countrymen would persist in pronouncing it as if written *odd*, and thus affording scope for petty joking. Others made matters worse by tacking a classical tailpiece to Reichenbach's Teutonic monosyllable, converting it into *odyle*. Thus the idea was insinuated that the unknown something was not a phase of energy, but a material organic compound, analogous to ethyle, methyle, propyle, &c. Before very long *od* was dropped. The physicists ceased to experiment for, on, or with it, and, despite the august sponsorship of Liebig, it was handed over to that semi-scientific limbo where exploded delusions await decomposition and

neglected truths their resurrection. Latterly, however, the Psychical Research Society have turned their attention to this subject, and have submitted Reichenbach's researches to a very careful and impartial revision.

The preliminary question may be raised, Why should the Psychical Research Society engage in an enquiry so purely physical? To this query it is not easy to give a satisfactory reply. Still the solution of a difficulty should be none the less welcome, by whomsoever it is furnished; all that we have a right to ask being merely—Is the solution genuine? The experiments undertaken by the Society, and described in detail at the meeting held in Willis's Rooms, April 24th, if not entirely decisive, have upon the whole an affirmative character, and in short justify the public in concluding that "there is something in it." In other words, our verdict must be given in favour of Reichenbach, unless some quite unexpected source of error is detected on further experimentation. The persons chiefly engaged in the research were Prof. Barrett, Mr. W. H. Coffin, Mr. E. Gurney, Mr. E. R. Pease, Dr. A. T. Myers, Messrs. F. W. H. Myers, H. N. Ridley, and W. R. Browne. The experiments were made at No. 14, Dean's Yard, Westminster, in a perfectly darkened room. The first point to be considered is whether the darkness was truly absolute? It was asserted that not a glimpse of light was perceptible to observers "even after an hour's immersion in the darkness." We understand, also, that the room was always darkened for a considerable time before the experiments began, so as to eliminate error from the possible presence of phosphorescent light. This precaution is the more necessary since our knowledge of phosphorescent bodies is far from exhaustive. From various considerations it is possible that most substances may be feebly phosphorescent. Hence the most satisfactory arrangement for the class of experiments in question would be a room permanently darkened. In the dark room was fixed an electro-magnet of the power of from 200 to 300 lbs. Conducting wires were carried to a commutator in the next room, and thence to a Smee battery of sufficient power. The room where the commutator was placed was separated from the darkened room merely by curtains, so that what was said in the one was distinctly heard in the other. In this darkened room, where was the magnet, were stationed, as we understand, Prof. Barrett, Dr. Myers, Mr. F. W. H. Myers, and Mr. Ridley. The commutator in the room on the other side of the curtains was in charge of Messrs. W. H. Coffin, E. Gurney, and E. Pease. It will be at once

perceived that the three last-mentioned gentlemen could, by making and breaking the current at such intervals as they thought fit, alternately create and annul the power of the magnet. Hence if a light was produced over the poles of the magnet on making circuit, and disappeared on interruption, it might be fairly concluded that the magnetism and the luminosity were causally connected.

The next point relates to the observers. Unfortunately not all persons are capable of perceiving this light. Of forty-five subjects tested, three only were able to perceive anything. Of these three the most successful were a Mr. G. A. Smith and a boy, Frederic Wells. It is right to add that both these had been to some extent engaged in mesmeric operations. It is, however, expressly stated that "both of them were entire strangers to these experiments, and disclaimed any knowledge of Reichenbach's work. In the first instance they were not told what to look for, but merely to note if they perceived anything amid the darkness, and if so, what, and where."

The English version of Reichenbach's work, by the late Prof. Gregory, is, we believe, out of print, and rarely to be met with. From the circumstance, however, that Dr. Ashburner repeated Reichenbach's experiments, the work is perhaps more known amongst persons who dabble in mesmerism than it is elsewhere. Hence, without pronouncing Smith and Wells to be tainted with imposture, we think it would be well if the experiments could be repeated with subjects not conversant with mesmerism.

We turn now to the experiments themselves, and consider in how far they demonstrate the reality of the phenomena sought for, and, as a necessary condition, how far they eliminate the possibility of trickery or collusion.

The operators being placed in charge of the commutator, and the observers being posted in the dark room, the sensitives, Smith and Wells, were introduced,—as it appears separately,—and after some time each declared that he saw a faint light or a luminous vapour. Each declared that this appearance was over the poles of the magnet, the one—the so-called north pole, or rather north-seeking pole—being the brighter of the two. The light was pronounced to be in the form of wavering cones, running to a point where touching the magnets and growing wider upwards. A current of air —*e.g.*, the breath—was said to drive the flame aside, but not to extinguish it. Various substances—such as a board, or a piece of velvet if laid in a flat position over the poles of the magnet—seemed not to affect the light, but it was at

once obscured if these objects were held between the magnets and the eyes of the "sensitives." If, as we understand, this interception was effected without their knowledge,—as would be quite possible in a dark room,—we have here valuable evidence of their good faith.

The most decisive proof is, however, the following: the sensitives always proclaimed that the light had disappeared whenever the current was interrupted. In a series of test observations made upon Smith, which lasted for more than an hour, contact was made or broken for fourteen times in succession. The intervals were intentionally varied from a few seconds to several minutes; yet, as the operators in the other room testify, Smith's exclamations, "It's there" and "It's gone," coincided exactly with the movements of the commutator. Hence it seems legitimate to conclude Smith saw the light when the current was established, but ceased to see it on interruption.

If this was not the case we must have either casual coincidence or indications obtained in some other manner. The supposition of casual coincidence Prof. Barrett disposes of very satisfactorily. There are in an hour 3600 seconds, in each of which there was an equal antecedent chance of the circuit being formed or interrupted. Hence the probability of even a single right guess is very small. But the chance against hitting fourteen right moments in succession, without a miss intervening, is so many millions to one that it may be practically dismissed as out of the question.

The more important consideration is whether the conduct of the experimentalists and observers, or any circumstances connected with the experiments themselves, might furnish the sensitives with a clue to what was taking place. On this subject we prefer to quote the exact words of Professor Barrett, in the "Philosophical Magazine." He says:—"Among such indications the so-called 'magnetic tick' at once suggested itself. Knowing precisely what to listen for, and therefore more keenly alive to the sound than Smith, who presumably knew nothing of this molecular crepitation, I failed to detect the faintest sound on the making of the circuit, and a barely audible tick on breaking contact was heard only when my ear was in close contact with the magnet or its support. This was due to the massive character of the magnet and stand, which also prevented any other discernible movement when the magnet was excited. Further I satisfied myself that, at the distance at which Smith stood from the magnet, it was impossible to discover when the circuit was completed by the attraction of any magnetic

substance about one's body : as a precaution, however, Smith emptied his pockets beforehand." Prof. Barrett adds, however, " At the same time it is quite possible a skilful operator, bent upon deceiving us, might be able to detect the moment of magnetisation and demagnetisation by feeling the movement of a concealed compass-needle. Against this hypothesis must be placed the fact that no information was given to Smith beforehand of the nature of the experiment."

The possibilities of imposture are further greatly lessened by the experiments made with the boy Wells. His perception of the luminous phenomena appears to have been more acute and sensitive than that of Smith. In the preliminary trials he saw the light earlier than Smith had done. The flame, according to his description, was larger and brighter than it appeared to Smith. To the latter, moreover, the light on breaking contact disappeared immediately, and he accordingly exclaimed " It's gone." To Wells it seemed to die away, rapidly but not instantaneously, and he therefore said, " Oh, you are spoiling it." Prof. Barrett here points out that there was still a large residue of magnetism left in the electro-magnet after the current was interrupted. We do not learn whether any experiments with permanent horseshoe magnets were tried with Smith ; but with Wells some very valuable results were obtained in this manner. He saw the luminous appearance on the poles of both : when they were silently removed without his knowledge he detected at once their new position. Says Professor Barrett, " Holding one of the magnets in my hand, Wells told me correctly whether I moved the magnet up or down, or held it stationary : this was repeatedly tried with success."

It was further ascertained that no effect was produced by these magnets upon a small compass-needle, even at one-tenth of the distance at which Wells stood. Hence the compass trick, to which Prof. Barrett referred, would in this case be ineffectual. Moreover, it is scarcely conceivable that a baker's shop lad would be familiar with this method of detecting magnetic action.

The next question which suggests itself concerns the compatibility of the luminous phenomena observed with known laws. We by no means mean to intimate that theories ought to override facts. But it will be granted that if some novel observation is found to be in harmony with our previous knowledge, we feel much better satisfied with its accuracy than if it appears irreconcilable and anomalous. If a luminous appearance hovers over the poles of magnets

it will not have arisen *de novo* as a creation of energy, but will be a work of transformation. In the case of the electro-magnet we have no difficulty: we have electricity converted into magnetism, and need feel little surprise if the molecular disturbance occasioned in the metal of the magnets, or in the air immediately in proximity to them, expresses itself as light, or as some other form of energy not yet known. Prof. Fitzgerald, F.R.S., refers to the well-known fact that the gases of the atmosphere differ in their magnetic relations, oxygen being strongly magnetic, whilst nitrogen and carbonic acid are decidedly diamagnetic. Hence a process of molecular disturbance in contact with a powerful magnet is to be expected. Again, it is well known that the atmosphere holds in suspension minute solids, which have also their magnetic relations. To determine whether either or both, or neither, of these relations come here into play, he proposes an experiment to which we shall shortly refer.

But whilst these or some kindred theory may probably account for the luminosity over the poles of an electro-magnet in full accord with the great truth of the conservation of energy, the case might seem at first glance dubious with a permanent magnet. But here the question is raised whether there is not a very slow loss of magnetic power gradually and constantly at work, the energy thus lost re-appearing as light, or, if anyone prefers the term, as *od*.

The "Reichenbach Committee" of the Society for Psychical Research promise that, when the simple fact has been fully established, they will proceed to a further examination of the nature and properties of *od*. With all due submission we doubt whether the simple fact will ever be accepted as established until some progress has been made with these investigations. Prof. Fitzgerald, in order to test his own suggestions, recommends observations with a magnet in a medium approaching as nearly as possible to a vacuum. To this might be added experiments in air freed from suspended solids by filtration through cotton-wool; in pure hydrogen, in oxygen, in carbonic acid, and in other gases and gaseous mixtures. Further points are the passage of the od-light through lenses of power and nature unknown to the sensitive observer, its reflection from plane and concave mirrors, by which means all possibility of trickery could be obviated. Not less important is the spectroscopic, polariscopic, and photographic examination of the light.

It does not appear that the Committee have as yet attempted to verify Reichenbach's original experiments with crystals, and on a number of other bodies. Thus there is indeed a vast

assortment of work awaiting them,—some of it, as we must admit, of no small difficulty. But whilst we would warmly encourage these investigations, and are prepared to welcome the results, whether the phenomena are traced to *od* or are merely novel manifestations of some form of energy already known, we must again express our inability to recognise their connection with Psychical Research, and still less with Spiritualism. As far as we see, there is in Reichenbach's phenomena nothing which need be unacceptable even to the most determined Positivist, or to anyone who regards the phenomena commonly attributed to soul, spirit, or mind as the mere outcome of organisation. True certain persons can perceive the magnetic luminosity, whilst others fail so to do. But this fact seems to us to stand upon the same plane as the well-known differences between man and man in the recognition of colours, in the perception of remote or minute objects, and in the hearing of very acute sounds.

II. ATLANTIS ONCE MORE.*

THE question of "lost Atlantis," which most of our readers will have considered as finally disposed of, has been reopened in a most remarkable work, which, whatever conclusion we may ultimately form, deserves a fair examination. The author undertakes to prove not merely that there once existed a large island, or possibly continent, where now extends the Atlantic ocean, but that it was densely populated and highly civilised, being the seat of a vast empire, with colonies extending from the Ganges and Central Asia to Peru and Vancouver Island; that it was suddenly submerged in a tremendous convulsion of Nature; and that all the culture of antiquity was due either to refugees who escaped the crisis or to settlers who had previously left the island.

It is plain that we have here a group of propositions not necessarily inter-dependent. If we grant a former land-

* Atlantis: the Antediluvian World. By IGNATIUS DONNELLY. London: Sampson Low, Marston, Searle, and Rivington.

connection between Europe and America, it does not follow, of course, that such must have still subsisted since the appearance of man upon the earth. Admitting the existence of Atlantis in the human epoch, we may still question its catastrophic doom and its claim to be the cradle of civilisation. In support of his conclusions Mr. Donnelly adduces a vast mass of evidence of the most varied kinds, and drawn from diverse sources. History, mythology, philology, not less than geology, zoology, and botany, have been ransacked with a wonderful amount of patience and enthusiasm, We shall not be surprised if, in the opinion of many of the outside public, he is held to have proved his point. The scientific world will naturally feel more sceptical. They will at once remark that direct evidence of the existence of such an island is wanting. Such eminent authorities as Mr. A. R. Wallace, Dr. Carpenter, Mr. Murray (of the *Challenger* Expedition), and Professors Agassiz, Dana, and Le Conte, conclude that the great oceans have occupied their present position, if not through all geological time, yet from an epoch prior to the origin of man. Perhaps our best course will be to examine the fourth chapter of the work before us, in which the author asks if the sudden disappearance of such a tract of country is possible? In reply to this question he recounts evidence proving the elevations and subsidences of large tracts of land, and the sudden havoc produced by earthquakes and volcanic action. But he seems to us to overlook the important consideration that the extensive elevations and submergencies were slow, gradual, and secular, whilst even the widest catastrophe wrought by earthquakes is in comparison extremely limited. Some of his authorities agree extremely ill with the conclusions they are adduced to support. Thus he quotes Prof. Geikie to show that "the earliest European land appears to have existed in the north and north-west, comprising Scandinavia, Finland, and the north-west of the British area, and to have extended thence through boreal and arctic latitudes into North America." Here, then, is a land-connection between the eastern and western continents, but not in the latitude assigned to Atlantis, which is placed about the Azores. "Within five thousand years," Mr. Donnelly writes, "the shores of Denmark, Norway, and Sweden have risen from 200 to 600 feet." So be it; but the occurrences of subsidences and elevations in certain places does not prove that such changes have occurred everywhere. They show, indeed, what *may have been*, but fall very far short of telling us what *must have been*.

In this section we find certain further misapprehensions. Mr. Donnelly writes :—" There can be no question that the Australian Archipelago is simply the mountain-tops of a drowned continent which once reached from India to South America. Science has gone so far as even to give it a name; it is called Lemuria." Now the hypothetical Lemuria was supposed to lie not in the Pacific, but in the Indian Ocean, extending from Madagascar to Ceylon, or perhaps Sumatra. The supposition of a continent reaching from India to the Peruvian coasts is exceedingly questionable.

Mr. Donnelly gives his opinion that " the Desert of Sahara was once under water, and its now burning sands are a deposit of the sea." This view is now, we may say, exploded. Dr. Lenz has found in the Desert fresh-water fossils, and its sands are considered to have been formed by the disintegration of the rocks, long exposed to intense heat by day, to a comparatively low temperature at night, whilst unprotected by vegetation.

We come now to the second part of the author's argument, where he seeks to show that there is nothing impossible or improbable in the sudden disappearance of an island like Atlantis. The convulsions and eruptions which he cites do not make even a remote approach. The most extensive is the subsidence of the fort and village of Sindree, on the eastern arm of the Indus, caused by an earthquake in 1819. This tract is 2000 square miles in extent. Now Mr. Donnelly gives the island of Atlantis a breadth of 1000 miles by a length of 2000 to 3000 miles. Hence its area would be from two to three *million* square miles! It is safe to say that we have no catastrophe on record which approaches even from afar the sudden disappearance of such a region. Further, the land about the mouths of the Indus is of exceedingly unstable character. The branches of the river often change their position to such a degree as seriously to interfere with engineering operations. Atlantis, on the other hand, is described as rocky and mountainous, not a stretch of alluvial sands and muds, such as are generally found in the deltas of large rivers.

We must therefore conclude that the sudden overthrow of Atlantis, if not impossible—a word which scientific men are not fond of using—is highly improbable. As far as this part of the subject is concerned we must return a verdict of "not proven."

We turn now to what the author entitles the " Testimony of the Sea." He lays great stress on the so-called " Dolphin " and " Challenger " ridges, the former of which

was found covered with volcanic deposits. He contends that "when these connecting ridges extended from America to Europe and Africa they shut off the flow of the tropical waters to the north; there was then no Gulf-stream; the land-locked ocean that laved the shores of Northern Europe was then intensely cold, and the result was the Glacial Period. When the barriers of Atlantis sunk sufficiently to permit the natural expansion of the heated water of the tropics to the north, the ice and snow which covered Europe gradually disappeared; the Gulf-stream flowed around Atlantis, and it still retains the circular motion first imparted to it by the presence of that island." He adds:—"It does not follow that at the time Atlantis was finally ingulfed the ridges connecting it with America and Africa rose above the water-level. These may have gradually subsided into the sea, or have gone down in cataclysms such as are described in the Central American books. The Atlantis of Plato may have been confined to the Dolphin ridge."

Here, then, we have a novel theory of the Glacial Epoch. Is it sufficient to account for the phenomena? It is admitted that the removal of the Gulf-stream would lower the temperature of Western Europe, which is now decidedly higher than might naturally be expected from its latitude. But for glaciation two factors are necessary—a low temperature and abundant atmospheric precipitation. Were the Gulf-stream suspended it is extremely probable that the quantity of rain, mist, and snow experienced in Britain, Holland, Denmark, Norway, and Sweden would decrease. Further, we have to account not merely for glaciation in Europe, but in North America, where it appears to have been, if anything, more severe than in the eastern continent, and in the southern hemisphere, where it has been traced as far as 3° south latitude. It is exceedingly improbable that the arrest of the Gulf-stream, or of any analogous current, should induce glaciation in Nicaragua and at Pernambuco.

It is scarcely conceivable that the Gulf-stream could retain for many thousands of years a circular motion imparted to it by an island no longer in being.

We come to the testimony of the Flora and Fauna. Mr. Donnelly points to the identity of the Palæarctic and Nearctic forms as an important piece of evidence in his favour. Admitting their close approximation,—which, indeed, leads some authorities to include these two great regions under one,—we must remark that it extends chiefly to the more boreal forms. A land-connection between the two continents in the genial latitudes of the Azores, Madeira, and the

Canaries, can scarcely serve to explain the presence of the reindeer, the musk-ox, the glutton, the lemming, &c., on both sides of the ocean. If, again, we compare the Fauna and the Flora of Africa with those of the Neotropical region, we find the most total contrast. This is not what we should expect if, as Mr. Donnelly holds, there was a land-communication between West Africa and South America or the West Indies. Whereas if, as is commonly done, we suppose animal life to have taken its origin in the Arctic region, as the part of the earth which first became sufficiently cool to be habitable and to have gradually extended southwards, becoming modified on its way, we should have, as may be seen, great similarity in the boreal regions, gradually passing southwards into extreme diversity. That a land-connection between Europe and America may have existed down to a comparatively recent period is regarded by geologists as not improbable. But the evidence at command places it much farther north than the supposed site of Atlantis.

An unfavourable piece of evidence may be drawn from the Fauna of the Azores. These islands, if Mr. Donnelly is in the right, must be remnants of Atlantis. Yet their animal population has nothing of a continental character, but seems formed of such waifs and strays as have been driven there by stress of weather.

Mr. Donnelly refers, in more than one part of his book, to cultivated plants as being either of unknown source or as being common to both hemispheres, and draws hence the scarcely legitimate conclusion that they must have originated in Atlantis, and have been introduced into other regions by the people of that empire in their commercial or military expeditions. As their empire, according to a map here given, included the coasts of India as far as the Ganges, Persia, Arabia, Egypt, and Nubia, on the one hand, and Mexico, the Valley of the Amazon, and Peru on the other, we may ask, How is it that they did not introduce the cultivation of sugar, tea, coffee, silk, and jute into their western possessions, and that of cocoa and cochineal into Africa and India? Yet these tasks were left for a much later age. The author asks, "Does Plato, speaking of fruits having a hard rind affording drinks and meat and ointments, refer to the cocoa-nut?" It is not impossible that he may have met with the cocoa-nut imported from India or the Eastern islands. We should think that its origin must be sought in the latter rather than in America. Be this as it may, there is no fruit so likely to spread without human agency as the cocoa-nut. It grows by preference on the sea-coasts; it survives long

immersion in water, and is not unfrequently washed ashore at places far distant from the lands of its growth. The mango, according to the most recent authorities, is a native of the Malay Archipelago, and has only been acclimatised in Continehtal India and in South America. One point we must here bring especially forward: it is said that the fruits which man enjoys are artificial products which have been slowly developed under the influence of cultivation. But the Malay Islands are remarkably rich in exquisite fruits, which are for the most part incapable of being acclimatised elsewhere. Might not this fact be held to prove that there, in the "Gardens of the Sun," is the earliest home of civilisation. It must be noted that Mr. Donnelly argues at some length, and with much force, against the supposition of a former land-connection between North America and North-eastern Asia, but on the possibility of animals and plants being introduced into South America across the Pacific he is silent.

He is inclined to seek the origin of our domestic animals in Atlantis, and he affirms that no wild species has been tamed during the historical ages, *i.e.*, the last 7000 years. We doubt whether much importance can attach to this observation, even if correct. The number of animals eligible for domesticity is not unlimited. But we may well question whether Atlantis could possibly be either the original home of the elephant or the place where he was first tamed. Palæontology teaches us that South-eastern Asia was the seat of the Elephantidæ. All historical accounts agree that this animal was first used in war in India, and that its employment spread gradually to Western Asia, North Africa, and Europe. The ass, in like manner, is due not to Atlantis, but in all probability to Africa, where certain closely allied forms exist in a wild state. The cat is also an African species, and her domestication has evidently spread into Europe not eastwards, but westwards.

The question here arises, why, if the aboriginal civilisations of America are of the same origin as those of the Eastern hemisphere, were the former devoid of the services of domestic animals? The horse, ass, cow, sheep, goat, and hog were evidently unknown in America at the time of the first Spanish settlements. Now, if these creatures were first tamed in Atlantis, why did not that empire, with all its commerce, navigation, and enterprise, introduce them into its American colonies?

With the historical, mythological, and linguistic evidence accumulated by Mr. Donnelly we cannot deal in full. We

must confine ourselves to discussing certain statements which are at issue either with known facts or with each other. We read on page 28 :—"Attention may here be called to the extraordinary number of instances in which allusion is made in the Old Testament to the 'islands of the sea,' especially in Isaiah and Ezekiel. What had an inland people like the Jews to do with seas and islands? Did these references grow out of vague traditions linking their race with islands in the sea?" Surely the Jews cannot be called an inland people, and since the maritime expeditions of Solomon they could not be altogether ignorant of islands.

On page 265 we read that the Egyptians had a horror of the sea, whilst on page 3 we are told of their circumnavigation of Africa, and on page 243 of a naval battle gained by Rhamses III., in the thirteenth century before our era, against a people called Tokhari.

Concerning the primitive religion of mankind Mr. Donnelly states (page 285) that we find a great Solar or Nature worship in the older nations. But on page 293 this is changed. We are told that "Men first worship stones, then other men, then spirits." Sun-worship here has disappeared. On the same page it is asserted that "Resemblances of names prove nothing." Yet resemblances of names are again and again appealed to, even when so slight as that between Olympus and Atlantis.

Concerning the Deluge of tradition we find on page 94 that "Egypt does not contain a single allusion to the flood." Turning to page 67 we meet with a very dissimilar account: —"The priests of Sais told Solon that before 'the great deluge of all' Athens possessed a noble race, &c. . . . So that the Egyptians, who possessed the memory of many partial deluges, regarded this as the great deluge of all.

On page 246 the Atlanteans are described as "a great maritime and commercial people, who carried vast quantities of copper, *tin*, and bronze (unalloyed by the lead of the South of Europe) to Denmark, Norway, Sweden, Ireland, *England*, France, Spain, Switzerland, and Italy." . . . . "Thousands of tons of copper and tin must have been introduced from Atlantis during the Bronze Ages into Europe." It is sufficiently bold to state thus the mineral wealth of a country which, if it ever existed, has been submerged for thousands of years. To proceed :—"There are no ancient works to indicate that the tin-mines of Cornwall were worked for any length of time in the early days." But on page 478 we read :—"In its markets (*i.e.*, Atlantis) must have met . . . . the *tin* of Wales and *Cornwall*, the *bronze*

of *Iberia*." It is not needful to point out how these two passages contradict each other.

The empire of Atlantis, as figured in a map opposite page 294, seems to have been put together on the principle of eschewing "scientific frontiers." In Asia it included the coasts of India from the Ganges westwards, the coasts of Arabia and Persia, the whole of Asiatic Turkey, and the regions around the Caspian and the Sea of Aral, extending eastwards nearly to the frontiers of China proper, but leaving Thibet, much of Affghanistan, and Northern India untouched. In Europe the Atlanteans held the coasts of the Mediterranean, the whole of Spain, Italy, and Greece, much of Austria and Switzerland, the South of Russia, the coasts of France, the shores of the Baltic, the southern half of Sweden and Norway, and the British Islands. There remained unconquered a wedge of land narrow in the centre of France, and getting broader through Germany, Poland, and North Russia. In Africa they held the Valley of the Nile, the shores of the Red Sea, the Mediterranean, and the Atlantic down nearly to the Gold Coast. In America their dominions were strangely grouped. Neglecting the very near and accessible Atlantic coast and the West Indian Islands, they occupied the whole of Central America and Mexico, the Valley of the Mississippi and its affluents, the southern shore of the Canadian lakes, and a line of country leading over the Rocky Mountains to British Columbia. In South America they avoided the entire northern and eastern coasts, but passing up the Amazon they overran the whole of its valley, and crossing over the Andes settled in Peru, Bolivia, and Chili! Never surely was a vast empire formed with such disregard of accessibility, desirability, and defence. Mr. Donnelly says that they avoided the "fever-stricken lowlands of Brazil." But, on his showing, they settled on the *more* fever-stricken coasts of West Africa, and in the *tierras calientes* of Mexico!

Of these colonies Egypt is said to be the oldest, and hence to have acquired most of the civilisation of the mother country. This is, in itself, exceedingly improbable. The first attempts of any country in colonisation are generally made in the nearest available places. The Greeks colonised Southern Italy; the Tyrians, Northern Africa; the Carthaginians, Spain; &c. Had the Atlanteans proceeded on this natural principle their earliest settlements would have sprung up in Spain, Portugal, and Morocco. But Mr. Donnelly could not shut his eyes to the manifest truth that in the basin of the Mediterranean civilisation has

proceeded from the east to the west. To account for this undeniable fact he is obliged to establish a colony of his Atlanteans in Egypt before they had attempted to subdue regions much nearer to their own country.

It will not be forgotten that he in his map includes the whole of Greece and the West of Asia within the Atlantean empire. But according to Plato's *Timæus*, in a document which Mr. Donnelly regards as historically authentic, and upon which he builds no small part of his argument, the Atlanteans had conquered Africa only up to Egypt, and Europe up to Tyrrhenia (*i.e.*, Tuscany). Then making a grand attempt to subjugate Egypt and Greece, they were finally defeated by the Athenians, and stripped of all their dominions in the Mediterranean. If this narrative, then, be authentic, Egypt can never have been an Atlantean colony.

The author's assumption that civilisation radiated outwards from the Mediterranean is not in all cases admissible. In Egypt it appears to have descended the Nile. We cannot carry further our survey of the speculations contained in this extraordinary work. But whilst holding that Mr. Donnelly has not made his case good, and that he relies too much upon that dangerous form of argument which may be generalised in the terms "Why may not A be X?" we yet think that he has raised a number of most interesting questions, especially in Ethnology. These, competent inquirers will find every way worthy of their attention.

III. A NEW THEORY CONCERNING THE BURNING MOUNTAINS.

By A. H. Swinton.

IT is interesting to note that the late prevalence of solar spots has inaugurated another eruption of historical Etna. The last period of eruption was in evident connection with the clearness of the solar disk in 1878, and the present statistics will eventually prove to stand similarly in connection with its spottiness in 1882. Often the periods of spots and no spots bring us to combined outbreaks from Etna and Vesuvius; it was so in 1689, 1694, 1723, 1755,

1759, 1783, 1787, 1805, 1809, 1832, 1845, 1868, 1876; and, as a rule, the eruptions from these and other burning mountains—Hecla, for example—will be found to cluster about the opposite solar phases that tread quickly on one another's heels, so that the cry of distress arising from the excessive summer rains still vibrates on the ear now winter has commenced to deluge the arable land.

Some consider that the winter season, the equinoxes, or summer solstice, and the full and new moon influence these paroxysms of the mountains. Prof. Silvestri telegraphs to the "Daily News" correspondent concerning the recent eruption of Etna:—"The coincidence with the full moon on March 23rd is again remarkable." Mr. B. G. Jenkins, in a letter to the same newspaper a year or two ago, observes—"If the movements of the lava in Vesuvius may be taken as an indication of what is going on in the interior of the earth, then the liquid fire within the globe must ebb and flow like the liquid ocean without, for the volcano, as your Naples correspondent has frequently observed, shows during its eruptions very many indications that the activity of the lava depends on the position of the moon." Certain members of the British Association—the names of Darwin and of Sir William Thompson of the Glasgow University occur to mind—have worked on these assumptions, and endeavoured to detect the existence of lunar vibrations with delicate seismographs, undaunted by the witticism that perchance they were keeping unconscious register of the local traffic. It must, however, be borne in mind that the statistics that originated this lunar theory were never unexceptional. Mr. Mallet, in introducing the subject to the notice of the Association, in 1859, remarked—"M. Alexis Perrey has thus succeeded, by the simple analysis of catalogues which he had previously drawn up, in proving, by three different and independent methods, the influence which the moon possesses in the production of earthquakes: first, that earthquakes occur more frequently at the Syzygies; secondly, that their frequency increases at the perigee and diminishes at the apogee of the moon; thirdly, that the shocks of earthquake are more frequent when the moon is near the meridian than when she is 90 degrees away from it." But Mr. Mallet added—"The numerical tables from which these three propositions are derived present some anomalies; and the author has omitted nothing to endeavour to account for them, and to prove the law which is revealed at their first inspection."

Passing from the influence of the moon to the certain

potency of seasons, we have yet something that needs clearing up. Mr. Mallet shows that with regard to earthquake shocks we have in the Northern Hemisphere, broadly speaking, a minimum of shocks in May, June, and the early part of July, and still fewer during September. Most shocks indubitably come in January, and many in August and October. Further, there are more shocks at the critical epochs of the year, the equinoxes and solstices. Here we might pause and enquire whether this lateral push, that throws the earth's surface into rolling billows, may not arise from heat expanding or cold contracting its surface; for what do we understand by the seasons but an increase or decrease of warmth? But it will be said, if the shocks that occur in winter preponderate over those which happen during the summer season, it must be cold, not warmth, that heaves the soil and vexes and pinches the fuelled entrails of the Typhœan monster that sleeps beneath the volcano. I had fancied so until the following solution of the paradox suggested itself:—

A little thought will convince anyone that the march of climate does not wholly depend on the progress of the sun north and south; we have an influx of cold or spells of warmth at every season of various duration. The fickle ice of winter may at any time break up, and cold shivering days and weeks arrive in June; more than this, the very tissue of the seasons is wove of such alternations, and Italy, no less than storm-swept England, is victimised by them. A sojourner at Naples, where the spring air is conceived to be ever balmy and delicate, thus writes on the 17th of last March:—"Long before it is time for the primrose to spread its petals, we have, at the very first sign of spring, not only the daffodil, but troops of bright oxalis, making sunshine on every green bank; then follow the sweet-scented violets, anemones, white starwort and composite flowers, that gladden the many-clefted hills of the Vomero and Posilipo. Balmy breezes fan our cheeks, and little birds flit twittering from tree to tree. All this delight we have already enjoyed is just now interrupted by the vagaries of mad March, who has turned the upper half of Vesuvius into a sugar-cone."

Climatic changes in Italy are most felt in the plains of Lombardy, which bear much the same relation in that respect to the central and southern portion of the Peninsula that Scotland does to the South of England. To these variations of temperature Prof. D. Ragona, Director of the Royal Observatory at Modena, has lately directed attention in a paper, "Sur les Périodes Annuelles de Chaud et de

Froid," published in the Transactions of the Association Française for 1881. The following is his method of establishing the periodical recurrence of the changes :—He first plots off the daily mean temperature of twenty years' observation, and draws a curve to represent the annual march of the temperature, increasing from January 10th until July 29th, and then decreasing. It thus is rendered manifest that the mean temperature rises and falls as compared with this smooth curve of hot and cold resulting from the advance and retreat of the sun, and that these rises and falls of temperature, broadly speaking, have a tendency to recur at the same times of the year.

Having the dates of some hundred eruptions of Etna and Vesuvius by me, I bethought me to compare them with the warm and cold periods of the year as determined by Prof. Ragona. Since, according to Messrs. Mallet and Perrey, we have more earthquake shocks during the winter than in summer, I fancied, as I previously intimated, to see the dates tally with the cold periods, whereas what was my surprise to see them all huddled together at the warm ones? I therefore transposed M. Ragona's two tables, so as to show the warm periods of the year only at Modena, and then compared them with one another and with my chronicle of eruptions, and I discovered a certainly general concordance. The following were my conclusions :—A period of warmth attended with increased activity on the part of Vesuvius or Etna may be looked for about January 8th, of a few days' duration; and another following the 20th of the month, terminating about February 8th. The next may be expected about February 12th, lasting until March 16th; followed by another commencing on March 21st, and continuing until April 6th. The next may be expected about April 24th, and the next at the commencement of May, the last indicating about two weeks of volcanic activity, followed by a pause, and then renewed activity about the 24th, continuing until the end of the month, or protracted into the first two weeks of June. Another period of warmth and volcanic activity may be expected to set in about June 20th, lasting some four or eight days. The week following July 11th may be expected to be one of warmth and volcanic activity, and likewise the two weeks following the 26th. Another period may be looked for about August 11th, lasting for about two weeks, and another the first week in September. The week following September 20th and the week following October 10th are marked by eruptions, and correspond to, but hardly coincide with, two periods of warmth in M. Ragona's first

computation, as may be said of a period of activity coming the first or second week in November. From December 1st until January 2nd volcanic activity seems to know but little respite, although it seems probable that outbreaks have arrived more frequently during the week following the 24th and about the 30th of the month.

If we now recal to mind the eruption of Etna just transpired, it might be an open question whether we should refer it to the influence of the full moon or to an influx of warm temperature. It will be remembered that the full moon arrived on March 23rd, and that the eruption began on March 20th and continued until April 4th. According to M. Ragona's second table a period of warmth should have been likewise looked for in Italy, beginning on March 20th and ending April 11th ; and, according to his first table, two periods of warmth should have come about this time,—one from March 27th to 29th, and another from March 31st until April 17th. Theoretically, his first table would with great probability assign the increase of warmth as the cause of the eruption ; but it must be remarked that, in this country, warm days did not set in until March 30th, and they terminated on April 6th, when the temperature in the shade fell, so that the eruption preceded the temperature change by nine days. The state of matters at Catania I am unable to divine: on March 21st it was reported in the newspapers that great cold prevailed there, and that the sky was cloudy, but when the change was first felt I do not know.

On the whole I think statistics show that the eruptions of Etna and Vesuvius come most frequently at the winter solstice, or about December 21st ; at the vernal equinox, or about March 20th; and somewhat less frequently at the summer solstice, or about June 21st : there are fewer in July, and about the autumnal equinox, or September 22nd, they are again frequent. Apart from the anomaly of the paucity of eruptions in July, these dates will be found indicated as warmer periods in the several seasons. As a corollary one might ask, Are thermometer fluctuations greater at the periods of sun-spots and no sun-spots when seismic activity is most violent ?

IV. WEALTH AS AN ELIMINATIVE AGENT.

By W. Mattieu Williams.

THE concluding part of the article on "Vice as an Eliminative Agent," in the last number of the "Journal of Science," is very suggestive. The writer says that the commercial scoundrel "robs not by crying 'Stand and deliver!' but by rigging the market. He belongs not to a gang, but to a syndicate, and when he has done more harm than a generation of bandits, burglars, or forgers, he dies in peace, and leaves his children in good positions."

Query—Are the positions of his children really good? Is the ill-gotten wealth with which he endows them likely to fit them for holding their ground in the general struggle for existence? Or may it be an effectual agent in exterminating the foul brood?

We must remember that the man who lives a life of vigorous commercial rascality, and is married to a woman of his own type, will transmit to his offspring a vigorous and vicious organisation. If his domestic partner is quite different, an alternation of type among his children will be the probable result, some resembling the father, others the mother.

What is the effect of the possession of unearned wealth upon young men with vicious predispositions? It is simply a weapon of "happy despatch," which operates by supplying abundant means of sensual self-indulgence. Given two young men with low animal organisation, and equal physical vigour and bodily health,—one a navvy or farm labourer, the other inheriting sundry thousands a year on coming of age,—which is likely to live the longer and leave the larger family? Anybody who has had opportunity of watching the career of representatives of these two classes can answer this question at once.

Only a small minority of human beings are benefited by inheriting large accumulations of wealth. An exceptional organisation is necessary to enable a young man to escape the temptations and physical excesses which the early inheritance of great wealth supplies. This is indicated by the degeneration and dying out of all aristocracies that are not continually recruited from the useful classes. The law is strikingly illustrated by our own race. While the working

Anglo-Saxon is crowding himself out from his old home, and overspreading the earth, the British peers—in spite of all their efforts to drag collateral branches into their titles, &c.—are utterly incapable of maintaining their numbers in the House of Lords, which would long ago have been reduced to a little camarilla of curiosities had it been dependent on the survival of the original stock.

An idle aristocracy bears the same relation to the general working members of the race that poodles and other lap-dogs do to bulldogs, collies, terriers, &c. The lap-dog is a canine aristocrat, supplied without effort with a redundance of physical luxury, and he perishes without the aid of any exterminating outside enemy. This is shown by the resemblance of King Charles's spaniels, &c., to our peers of the realm. In spite of the demand for these fancy animals, indicated by their high price, the maintenance of the breed is a matter of great difficulty.

I have noticed a curious physical resemblance between them which is very suggestive. The general laxity of fibre in the petted spaniels and poodles of "pure breed," *i.e.*, with blue blood, is indicated by the protrusion of the tongue, owing doubtless to lack of muscular tension. The Dundreary lisp that really does prevail among their biped analogues (though not to the extent represented by their caricaturists) is evidently due to a similar falling forward of the tongue. The simple experiment of imitating this defect of articulation will show that it is caused by a protrusion of the tongue, in such wise that its point languidly strikes the teeth when it should vigorously press against the palate.

A visit to the House of Lords, and careful observation of the tones of voice, the gait, the lolling on cushion, the nonchalance, and other movements of the surviving representatives of ancient aristocratic families, will supply further evidences of the analogy above named.

My own observations (so far as they go, but this is not very far) on the *moral* characteristics of the survivors of a long-sustained aristocratic line indicate in this respect a very high type, shown by lofty forehead, high coronal region, and that polished and true courtesy due to genuine regard for the feelings of others which presupposes the easy suppression of passionate impulse and self-assertion. These I find to be combined with simplicity of taste, and very moderate indulgence in eating and drinking.

Other characteristics that might be named further indicate that, in the struggle with the terrible temptations presented

by their surroundings, the grosser animal specimens, the more passionate and sensual, have succumbed, while only those of a higher moral type have been able to survive; but even their survival has been limited by the absence of those motives to physical and mental effort which have fed the energies of their more favourably situated fellow-countrymen.

The *working* politicians among our aristocracy, and the very exceptional few who have distinguished themselves by earnest scientific or literary labour, are of course exceptions to the above. These have reverted to the natural conditions of vigorous human development, conditions enforced upon the mass of mankind by the fact that our food does not grow ready cooked at our feet, nor our clothing on our backs.

Modern Royal families, in constitutional countries like ours, have such heavy social duties, and responsibilities and restraints forced upon them, that they may be fairly included among the "working classes."

V. THE ACTION OF POISONS ON THE PETALS OF FLOWERS.

By A. Anthony Nesbit, F.C.S.

Continued from Vol. IV., p. 743.

Alcohols employed: Ethylic, Propylic, Allylic, and Amylic.

ALL these extracted the chlorophyll from the stems of the plants, but in varying quantity. Propyl, allyl, and ethyl alcohols extract a large quantity, and amylic very little. Propyl, allyl, and amyl alcohols are far more destructive to the life of the flower than ethyl alcohol. The flowers in these alcohols would show signs of fading in an hour and a half, whilst those in ethyl alcohol would remain quite healthy six or eight hours.

When ethyl alcohol was diluted its action on the flowers became less. If diluted from one to three, or from one to five, the flowers after twenty-four hours' immersion would

be but little more faded than those which were kept in water only, and if diluted down to one to ten the flowers would remain about the same time as those in water.

Action of Boric and Salicylic Acids.

Solutions ranging from 1 to $\frac{1}{4}$ per cent of crystalline acids were used. Salicylic acid always discoloured the stems, whilst boric acid did not. All the flowers in salicylic acid were much more withered after twenty-four hours than those in water alone. All the flowers in boric acid were withered more than those in water, but not so much as those in salicylic acid.

Acids and Alkalies.

The following solutions were used, *viz.*, normal carbonate of soda, normal potash, and normal sulphuric acid. All the flowers were withered after some time, one as much as another. The stems of those in acid were discoloured; those in alkalies were not.

Action of Mineral Salts.

Salts which caused the Stems to turn Brown.

Copper sulphate.
Mercuric chloride.
Potassium sulphocyanide.
Ferric chloride.
Ferrous sulphate.

Salts which had no effect on the Stems.

Lead acetate.
Sodic arseniate.
Potassium chromate.
,, bichromate.
,, ferrocyanide.
,, ferricyanide.
Sodium sulphite.
Zinc sulphate.
Barium chloride.
Strontium nitrate.
Permanganate of potash.
Oxalate of ammonia.

Salts which coloured the Flowers.

Potassium chromate.
,, bichromate.
,, ferrocyanide.
,, ferricyanide.
Ferrous sulphate.
Ferric chloride.
Copper sulphate.
Potash alum.

Salts which did not colour the Flowers.

Potassium permanganate.
And all those salts which form a colourless solution with water, with the exception of potash alum and other salts of alumina.

Potassium chromate and bichromate are about equal in their action on sweet peas. Both colour the petals strongly.

Ferrocyanide and ferricyanide of potassium are equal in their action, and do not differ appreciably in destructiveness from the chromate and bichromate. Both colour the petals, the tint being the same in both.

Cyanide and sulphocyanide of potassium do not differ one from another with regard to their action on sweet peas, but are more hurtful than the chromates and ferrocyanides.

The salts of iron seem very hurtful to the life of sweet peas, more especially the protosulphate of iron. Both colour the petals, the colour being brownish black. The ferrous sulphate colours the petals more strongly than the ferric, and kills the flowers sooner.

Strontium nitrate and barium chloride are less hurtful than the chromates; the ferro-cyanides and the cyanides also much less hurtful than salts of iron and zinc.

Oxalate of ammonia is more destructive to sweet peas than sulphate of zinc or sulphite of soda.

Of copper sulphate, mercuric chloride, arseniate of soda, and acetate of lead, the least hurtful is mercuric chloride, the flowers remaining quite fresh for eight hours sometimes. Arseniate of soda is the least hurtful, while lead acetate and copper sulphate seemed to be equal in their effect and less hurtful than arseniate of soda.

VI. HAECKEL, HUXLEY, AND MIVART ON THE SCOPE AND PROVINCES OF ZOOLOGY.

By Oswald Dawson.

PROFESSOR HAECKEL, in a volume recently contributed to the International Library of Science and Freethought, appends to an address delivered at Jena University in 1869, and thrice published in Germany since that year, a table illustrating the scope of zoology which contains several words differing from, or foreign to, those

employed in the terminologies adopted in this country. In some half-dozen points this table further differs from that given in the author's "Generelle Morphologie" in 1866. The word "œcology" is common to both; it is also defined in the "History of Creation" and in the "Evolution of Man," and thus has claim to familiarity, though Professor Karl Semper, who undertook an examination of "Haeckelism" some time ago, and contributed to the "International Scientific Series" a volume purely œcological, ignores the word, employing in its stead the cumbersome phrases "Universal Physiology" and "Physiology of Organisms." The difference in the two terms lies in this: Professor Semper's "Physiology of Organisms" embraces dispersal, but seemingly not Chorology. Professor Haeckel's Chorology embraces dispersal, though his definition of œcology entitles it to include the latter. It runs as follows (*vide* the address):—"By œcology we mean the study of the œconomy, the housekeeping of animal organisms. This has to do with the totality of the relations of the animal, both to its inorganic and organic surroundings, and, above all, the amicable and inimical relations to those animals and plants with which it comes into direct or indirect contact: in a word, all those complex mutual relationships that Darwin has shown are the conditions of the struggle for existence." In the "History of Creation," œcology is stated to concern "especially the circumstances of parasitism, &c." Why should œcology concern *especially* the relations of living beings to *organic* surroundings? In his Stettin address, not less historical than those delivered at Munich and Eisenach, Professor Haeckel said in 1863 (*vide* p. 25) "Many gaps and weak places in the growing edifice give large opportunity to the attack of foes. On the other hand, many circumstances are still altogether, or almost altogether, unknown to us that are possibly of no less moment in the origin of species than Natural Selection in the struggle for existence, on which overstress is, perhaps, laid by Darwin. *No less important* than these must be those external conditions of existence *of the inorganic world* that are too much neglected by Darwin;" or to utilise the analogy, it may be a *housekeeper's* business to be affable to visitors and hostile to burglars, but not "especially" so—not more so than to prevent a perpetual draught invading a much-frequented passage or speedily to renovate disordered drains. Œcology, therefore, includes the relation of animals and plants to the wind and other "means of dispersal." Profitable as it may be for original workers to read chorological facts in the light of œcological and physiographical

speculations, that union cannot with propriety extend to a tabulation of the provinces of zoology. The African deputation of *Polypterus* may incite to a little speculation as to the "how," for can it be declared that the *Polypterus* of the Senegal and of the Upper Nile are independently originated by homoplastic agencies when we reflect on the extensive similarity of the piscine fauna of the Upper Nile and West Africa? Yet the comparative chorology of the Dipnoi and Osteoglossidæ (*vide* "Study of Fish," p. 223) is a topic surely worthy of being discussed without reference to physiographical changes or the œcology of dispersal. "Distribution" and "œtiology" share the last chapter of Professor Huxley's "The Crayfish." It is divided into nine sections, of which the first six treat of distribution, *diffusion not being here discussed, consequently Professor Haeckel's "Perilogy" and its competitor, Dr. Mivart's "Hexicology"* (terms affiliating the study of environment generally and therefore of dispersal, to Chorology), *are not needed.*

Professor Haeckel and Dr. Mivart employ two other rival words. The footnote to page 2 of "The Cat" runs thus:—"The word Psychology is here used in its wide and (as the author believes) in its proper sense as embracing Physiology within its scope," and Chapter xi., §§, 5 17, demonstrates that Dr. Mivart means what he says. Professor Haeckel's new word "Ergology" comprises Physiology and Psychology. The "Physiology" of "The Crayfish" enters the realm of "Psychology." Dr. Letourneau, contributing the volume "Biology" to the Library of Contemporary Science, states:—"Under the designation 'Biology' we merely place the exposition and the co-ordination of all the great facts and great laws of life, or nearly what is usually understood by "General Physiology," when this denomination is applied to the two organic kingdoms. In this volume we have simply attempted to state concisely what life is, and how organised beings are nourished, grow, are reproduced, move, feel, and think." We thus have a choice of four words—Biology, Physiology, Psychology, and Ergology, the adoption of any one of which means that splenic undulations and *felo de se* fall within the same province; the study of innervation, reflex action, consciousness, &c., furnishing a series of gradations from the one to the other. Notwithstanding, some may be disposed to adopt Physiology and Psychology as primary and co-equal provinces, a procedure sufficiently pardonable to deserve refutation here. Omitting for the moment the question of consciousness (which may belong to "*metaphysics*"), under which province are the various other

subtle forces pervading organisms to be ranked? to specify a few. There are the properties with which the late Mr. Darwin's renowned Pangenesis Gemmules are endowed; there is the "kind of infection" which prompts a bone to go "out of its way, so to speak, to become cartilaginous before being ossified" ("Balfour," vol. ii., p. 494), and the analogous influence by which Professor H. G. Seeley would explain the multiplicity of mandibulan ossifications (*vide* "History of the Skull," King's College (London) Scientific Society, 1882); finally there is the *psyche* itself, the *psyche* of "*the* philosophy of *the* philosopher," according to Dr. Mivart, which, as embellished by that philosopher, proves to be the agent employed by the Creator, who, making use of living organisms as means," evolves "new concrete forms" by a process so subtle that "we cannot imagine how they are produced;" the *psyche* at once the antagonist of natural selection, pangenesis, perigenesis, dramatic and fictitious sensationalism; the veritable *psyche*, which "there is no adequate reason for believing" to be other than identical with the *psyche* for whose survival of the dissolution of *man's* body, "there is good evidence of a philosophical kind."

In the "Evolution of Man," Table xxix., Prof. Haeckel names the central nervous system the *psyche*; the dorsal marrow, *noto-psyche*; the parts of the brain, proto-, deuto-, meso-, meta-, and epi-psyche, though, in the text, he names the "medulla capitis," *encephalon*. "Physiology" has a no less variable meaning than the *psyche*; it has an etymological claim to concern itself with all science; ordinarily it is synonymous with the "Institutes of Medicine"; according to Prof. Huxley ("Anat. Inv. Animals," p. 26, and "Ency. Brit.," 9th ed., article—"Biology"), it embraces œcology, which Prof. Semper entitles Universal *Physiology*, or the *Physiology* of Organisms; Dr. Mivart finds ("The Cat," p. 389) that phylogeny "may, by a remote analogy, be termed 'the *physiology* of the species'"; finally, in Prof. Haeckel's table, it includes the *physiology* of "soul-life" and of imagination, while chorology is embraced by the "*Physiology* of Relation. In the table in the "Generelle Morphologie," the physiology of relation (internal) is classed with what Prof. Haeckel now terms "Perilogy," while the physiology of nutrition and generation are classed together. In the new table, though Perilogy alone constitutes the physiology of relation, the physiology of sustentation and generation are still classed together, and only secondarily associated with the physiology of the animal functions. Dr. Mivart adopts this arrangement ("The Cat," p. 386); in

"The Crayfish"; reproduction and the physiology of the animal functions share the same chapter. "Splanchnology" is the name applied to the study of the structure and functions of the organs of sustentation and generation; neurology to the nervous system; in Table xxvi. of "The Evolution of Man," the muscular and skeletal system are classified together, though no designation, so far as I am aware, embraces the study of osteology and myology, nor unites them to splanchnology, thus bringing the intermediate position of neurology between these departments and "Psychology" into perspicuity.

Prof. Haeckel divides Biology into Zoology, Botany, and Protistics; therefore, when the ownership of a particular organism is a matter of contention between the zoologist and botanist, the creature remains a Protist until the fluctuations of taxonomical diagnostics afford an opportunity to relegate it to the custody of one or other of these investigators. By a somewhat analogous procedure, neurology, together with the study of the various subtle forces pervading organisms above specified, together with those of phrenologists and spiritualists, with any others which may be discovered, re-discovered, or invented, may be considered as occupying an intermediate position between the study of all body functions, save those of nerve on the one hand, and the study of intelligence on the other hand. Psychology *overlaps*, though, as Dr. Mivart insists, more strictly *includes* these studies. Nevertheless, for convenience sake, the entire *work* performed by an animal or plant, may be termed—as a briefer form of "Psychology in its wide sense"—"Ergology."

It is surprising to see "Anatomy" honoured in the table when one reflects upon Prof. Haeckel's remarks on the assumption of "zootomy" being anything other than a pure art; it happens the two words labour under a precisely equivalent etymological disqualification. It is under this subdivision that there occur four words foreign to the works so often quoted in this article. One of the four is defined in *Gegenbaur*; another one in *Macalister*, here possessing a different signification; one is misprinted "Cosmology," to the consternation of anyone not familiar with the proper word; one, "Blastology," is, I believe, the equivalent of the "Prosopology" of the table in the "Generelle Morphologie," and already pre-occupied for the study of the countenance; the definition of one subdivision is almost that of its parent —Anatomy being study of body-structure, and Tectology, study of structure. None of the four are given in the index, and not one of them explained or mentioned in the Address

to which the Table is appended. Two secondary subdivisions occurring in the Table of the "Generelle Morphologie" are absent from their corresponding places here, while under Anatomy is placed, in brackets, "Comparative Anatomy." Prof. Haeckel's second subdivision of Morphology is of a kindred nature: ontogeny is defined as "history of embryo," a delusion long ago dispelled by Prof. Haeckel himself; metamorphosis (larval) is written instead of "Schadonology"; "Metamorphology," a term as unsuitable as "Promorphology," and occurring in the table in the "Generelle Morphologie," is here ignored, while "Anaplastology," "Metaplastology," and "Cataplastology" (defined "Gen. Morph.," band ii., s. 22), are likewise excluded; lastly, under Phylogeny are bracketted the three words,—Palæontology, Genealogy, and Classification.

We pass, without reluctance, to "The Crayfish," to analyse the morphological chapters therein. "The Crayfish" is, "in a sense, a house, with a great variety of rooms and offices" (p. 139). "That which architecture, as an art conversant with pure form, is to buildings, Morphology, as a science conversant with pure form, is to animals and plants" (p. 140). "The entire body of the animal, when reduced to its simplest morphological expression, may be represented as a cylinder, closed at each end, except so far as it is perforated by the alimentary apertures." A metamere is described, and remarks made with a view to exposing the teleological blush of serial homology (pp. 149, 174). Histology is treated, and also embryology; in fine, the animal is studied simply as if it were the work of an architect and his builder, *the analogy being complete*. We have cells and calcareous salts; we have the bricks and tiles; we have the organs, so the rooms; the antimeres, as exhibited in symmetrical houses; the metameres, as better illustrated by a row of houses; finally, we have the stereometry of organisms (*vide* "Gen. Morph.," band i., Buch iv., Tafeln. i., ii.), so we have the style and plan of the house. Prof. Huxley treats these topics apart from Comparative Morphology. This essentially *architectural morphology*, therefore, may appropriately be designated "Tectology." "Antimerology" and "Metamerology" (Table, "Gen. Morph.") link Promorphology (stereometry of organisms) to "Merologie" (study of parts, Bd. i., s. 25 and s. 29), this being an extremely valuable word.

Prof. Haeckel's elevation of ontogeny to the post of sole companion of phylogeny (in theory, though not in practice) is paralleled by Prof. Huxley's esteem for palæontology,—

"the only perfectly safe foundation for the doctrine of Evolution" ("Anat. Inv. Animals," p. 41). Those who wish to perceive the consequences of this belief may peruse the last section of the chapter on Comparative Morphology in "The Crayfish," and the chapter on Taxonomy in the "Anatomy of Invertebrated Animals." In the latter, after using such phrases as "the closest affinities," "specially modified anenterous *Nematoscolisces*," "no annectent forms between," "natural to suppose that the first process preceded the second in order of evolution," Prof. Huxley writes: "It is very important to remark that these morphological generalisations, so far as they are correctly made, are simple statements of fact, and have nothing to do with any speculations respecting the manner in which the invertebrated animals with which we are acquainted have come into existence. They will remain true, so far as they are true at all, even if it should be proved that every animal species has come into existence by itself and without reference to any other." Similarly, in "The Crayfish," we read of "degrees of affinity" and of "morphologically related," and are then told that "no amount of purely morphological evidence can suffice to prove that the forms of life have come into existence in one way rather than another." No; nor any amount of chorological evidence; nor if Prof. E. D. Cope lives to describe billions of fossils; nor if pigs begin to fly, and fishes forsake the sea, and every ape in the world invent a razor, shave, and serenade his mistress, will phylogeny (pre-historic) be *proved*. It may be very interesting to construct Taxonomies in the form of a Tree, to discourse on affinities and degenerated forms, and then to declare, "Oh, this means nothing; it is simply a mode of stating the truths of anatomy and embryology; wait till certain requisite fossils are discovered in certain strata, and *then* it will become a 'phylogeny'," but of the two evils I prefer dogmatism. Let an eternal barrier be placed between the two species of Taxonomy—"Phylogeny" and "Phylotecty." There is nothing derisive in this latter word; it simply expresses the phylotectist's adherence to the *architect* theory.

If the comments made in this article are unimpeachable, there are five primary provinces of Zoology (not given *in any particular order* below:—*Œcology*, or the study of the environment of an animal; *Ergology*, or the study of the *work* performed by an animal; *Chorology*, or the study of the distribution of animals; *Tectology*, or the study of the materials of which an animal is built, and of the plan upon

which it is built; *Taxonomy*,* or the study of an animal's "place in Nature," which Taxonomy may be either "*Phylogeny*" or "*Phylotecty*."

In separating Œcology from Chorology, Prof. Huxley has been followed; in divorcing it from Physiology, Dr. Mivart; in dividing Ergology into three instead of two sub-provinces, justification is sought by referring to a more or less parallel classification of the departments of Biology by Professor Haeckel, namely, into Zoology, Protistics, and Botany, the sub-provinces of Ergology being—(i.) Physiology *minus* Neurology; (ii.) Psychology; (iii.) "Nousology" (?).† In deference to etymology and convenience, Tectology has been extended so as to embrace Promorphology, or the Stereometry of organisms: the word "Phylotecty" has been invented in antithesis to "Phylogeny," thus bestowing a *prestige* on the speculations of a school of teleologists whose consciousness of their philosophical and numerical jeopardy will not allow them to fail to appreciate the compliment. One point alone remains to be contested.

The last chapter of "The Cat" is devoted to a consideration of the "pedigree and origin" of that animal. Phylogeny concerns the former, no special designation is given to the study of the origin. The treatment of this latter commences thus:—"This second question refers, as before said, to the probable causes which have determined that process of evolution which has, in fact, taken place." No doubt this procedure is in accordance with Professor Haeckel's views. Professor Huxley has borrowed from the physicians the word "Œtiology," and this "œtiology" is no insignificant department of occasional value, but is one of the four primary provinces of the science. In the "Anatomy of Inver-

* "Taxonomy" would be better restricted to the actual tables of classification, save that we have no designation for the aggregate study of Comparative Merology, Embryology, &c.

† νους, contraction of νόος, talent, disposition; "νόος is sometimes used by Plato for the excercise of reason" (Pickering, and Dunbar—Lexicons); ἀνθρώπων νόος, the mood or temper of men (Liddell and Scott, abridged, 1852); νοσοφαλής, weak in mind, insane. *Nous.*—"This Greek word has become thoroughly naturalised" (T. Lewis O. Davies, "Suppl. English Glossary"). Given in "Imperial Dictionary," 1882. It occurs in "Peter Pindar, in the "Ingoldsby Legends," and in Hood's "Fairy Tale"; νους, as reason (the faculty), universally used; νόησις, also a definition (C. D. Younge, "Eng.-Gk. Lex.," 1874). "The sum total of the mental action of a rational animal may be called its *noesis*" (St. George Mivart, "The Cat," p. 386). *Noesis* is given in *Mayne* as including *perception*. Thus both *nous* and *noesis* are borrowed without modification from the Greek, but the former is as familiar to the populace as is the study of *Comparative* anecdotal *Psychology* itself.

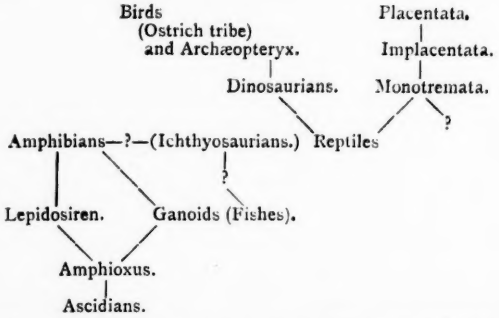
tebrated Animals" under this section the topic of abiogenesis is discussed, three causes of variation specified, and phylogeny is defined. In "The Crayfish" under this heading, the creation and abiogenesis (of crayfishes) are discussed, or rather dismissed; also diffusion, changes in physical geography, distributional difficulties, fossil *Astacomorpha*, and the genealogy of crayfishes. In the article "Biology" in the "Encyclopædia Brittanica," also in the "Anatomy of Invertebrated Animals (p. 35), reference is made to "the attempt to found this branch of science made in the "Origin of Species." Many people will suppose that any further critical comment upon a word boasting so celebrated a patronage must be impertinent. Nevertheless, to test whether or no Professor Huxley is or is not exaggerating the scope of œtiology, as indicated in the "Origin of Species," cannot be deemed superfluous. The account which Mr. Darwin gave of the origin of species reminds one of that given by Moses, the chief difference lying in this: that while in the writings of the latter on creation we have an abrupt and discernible transition from the Elohistic to the Jeovistic document*, no such harmonious leap is manifest in the "Origin of Species." Mr. Darwin detected this, and accordingly declared that "this whole volume is one long argument." Professor Huxley described the "Origin of Species" as "a mass of facts crushed and pounded into shape, rather than held together by the ordinary medium of an obvious logical bond. "But Professor Huxley adds, "Due attention will, without doubt, discover this bond, but it is often hard to find" ("Lay Sermons," p. 257). The "Origin of Species" *is* an œtiology indeed. The sub-divisions of that department are blended in the most inextricable confusion, and with equal discriminations and modesty the doctrines of "descent with modification" and of "natural selection" are labelled "my" theory and "our" theory (pp. 291, 308, 310, 312, 344, 351, 359, 362, 375, 379). At other times we hear, with more propriety and equal indiscrimination, of "the" theory and "this" theory (pp. 265, 279, 286, 289, 313, 409, 429). Here choice is offered between the theories of creation and of natural selection (*vide* esp., p. 383); there between creation and descent. Criticism on the "Origin of Species," that is to say the body of the work, must be regarded as fundamentally distinct from complaints about the title. The "Origin of Species" is a legitimate term com-

* Genesis: Its Authorship and Authenticity. By CHARLES BRADLAUGH. Part I. Third Edition. International Library of Science and Freethought (vol. iv.), 1882. *Vide* pp. 9-11; 93-95; 323-328.

prehensible to anyone who *will* take the trouble to recognise that it means the *furtherance* of variations, and not their *origin*. "Natural selection" is "justly and properly to be termed" thus (*vide* "Lessons from Nature;" by St. George Mivart, Ph.D., F.R.S., &c.; London, 1876, p. 290), *if* one can only grasp the conception that it is but a synonym for the "survival of the fittest" in the "struggle" for *existence*; but does history since 1859 tend to show that that is such an easy task? While even the "struggle" receives a little countenance *if* one will but promise to remember that it is used in "a large sense," "metaphorical sense," "a far-fetched sense," and "in these several senses," "for convenience' sake" (*sic*) (*vide* "Origin of Species," p. 50), but there is much virtue in an "if." But the applauder of Charles Darwin may attempt to demonstrate this to be a parade of cavilling by reminding us of the need of economy of space so often insisted upon in the "Origin of Species." He may plead that whenever Mr. Darwin speaks of "the theory of descent with modification," it is only from "sheer want of room" that he does not in *every* instance append the words "through variation and natural selection," as he does on pp. 313 and 404, and the applauder's position will be unassailable, for until Mr. Darwin's life and correspondence are published it is superfluous to attempt to decide whether we have to deal with a hypocrite or a simpleton.

Until a week or two ago I intended to here expose a few of the self-stultifications which the applauders of Mr. Darwin are so prone to exhibit, to comment, for instance, on the alleged courtesy to opponents, and justice to Mr. Alfred Russel Wallace, forsooth; but it occurred to me that the first-fruits of œtiology need no supplementary exposures. Let us therefore examine how far phylogeny and other divisions of "œtiology" harmonise together. Passing over Mr. Darwin's speculations on Abiogenesis, let us assume that, from the pedagogic stand-point at any rate, Phylogeny has priority to the other branches of "œtiology"; granted this, it is accordingly desirable to have *one or two* tolerably "dogmatic" genealogical trees. A Professor of Biology has recently made Mr. Darwin's death the occasion to repeat his conviction that Prof. Haeckel's phylogenies are too positive. His remarks are immediately succeeded, in the same paragraph, by a sentence containing the words "Darwin's decisions so weighty." It is not stated that the latter expressed the opinion that Prof. Haeckel had "thus boldly made a great beginning"; that "he who wishes to see what ingenuity and knowledge can effect, may consult

Prof. Haeckel's works"; that, "as we have no written pedigrees, we are forced to trace community of descent by *resemblances of any kind*. Therefore we choose, &c."; that it is "almost impossible" to study phylogeny without "the aid of a genealogical tree"; and that Prof. Haeckel triumphantly cited Mr. Darwin's agreement with "the general outline of the genealogical tree," as given by himself ("Origin of Species," p. 381; "Descent of Man," p. 158; "Origin of Species," p. 372 and p. 379; "Evolution of Man," vol. i., pp. 103, 104). Consequently, for the benefit of those who approve of uniting phylogeny to the "œtiological" portions of œcology and psychology, I will construct the phylogeny of the *Chordata* from Mr. Darwin's chapter on Affinities and Genealogy. Indeed, it is "almost impossible" to avoid so doing; therefore, *vide* "Descent of Man," second edition, *revised* and augmented; preface dated September, 1874.



In fine, therefore, there seems reason to adhere to the method of Dr. Mivart in respect to the isolation of Phylogeny; œtiology being a science—"wheels within wheels," fit first-fruits of a book comparable to a "cogwheel" striving to articulate with a fellow wheel of smooth circumference. Of course the theory of natural selection is not affected by any considerations as to the knavery or naiveté of one of its propounders, or the self-stultifications of his applauders. Discussion on the latter may be, perhaps, outside the sphere of a Journal of Science, while the maturity or puerility of natural and sexual selection affect not even remotely the nomenclature, propriety, or the reverse, of the provinces of Zoology:—*Ergology*, *Tectology*, *Taxonomy* (provisionally), *Chorology*, and *Œcology*.

VII. NOTES ON THE MIAS.

By W. de Crespigny, of Sarawak.

TAILED men are even to this day occasionally mentioned in your Journal and considered to be (if found) a missing link between men and monkeys, the fact being lost sight of that the ape is a link between those two animals, and that the link sought should have no tail.*

While upon the subject of apes a few remarks on the mias (pr. *maias*), or orang utan, may be permitted. During my twenty-six years of life in the Malay Archipelago I have had many opportunities of studying them, although I have met with only one species. They live *in* the forest, among the branches of the trees, and very seldom come to earth. I have never seen one on earth alive and free, and believe they only come down when progress aloft is impracticable, owing to the distance between the branches of neighbouring trees happening to be too great; and this in the forests of Borneo is *very* seldom the case, for the trees grow as close as Nature will allow them to. They live on fruit. They form families of male, female, and infant; and I have seen not only one infant, but also that of the previous year included in the group. They make nests in the fork of the tree, or in the fork of a branch of it, of transverse pieces of stick, but they do not thatch them, as some suppose, although they do bestrew the interior with leaves. The female and infant sleep in this nest, the male higher up. The nests are occupied only for a few days; that is, until the fruit in the immediate neighbourhood is consumed, when they move on. The arms of the orang utan (men of the forest, if both words are considered as substantives; wild men, if utan is used as an adjective, which it seldom is) being very much longer than its legs, it trusts mostly to them for its powers of locomotion; and I need scarcely add to what has gone before that the picture of a maias (or its skeleton) as represented in the Natural History portion of the "English Cyclopædia," where the animal is represented to be walking upright on its feet with the aid of a staff in its hand, is misleading. I have read that once when a maias was killed,

* We have repeatedly pointed out that the anthropoid apes are as tailless as man. The instances of tailed human beings we regard merely as reversions. —Ed.

which was destitute of thumb-nails, it was considered as belonging to another species. This is not so, but the thumb-nails become obliterated as the animal increases in age, owing to its continual travels through the forest, and the constant use they are put to in tearing open obdurate, because unripe, fruit. I have seen many men in Borneo whose nails, in consequence of life and work in the forest,—such as searching for gutta, ratans, and edible nests,—have become almost obliterated.

The pouch or drum under the chin is, I take it, not a drum, but really a pouch, or, as the sailors call a similar process in monkeys, a "panam bag," or receptacle for storage of food not required for immediate use. True I have never found food in this pouch, but, on the other hand, I have never heard the maias roar. While it happens that the maias has really no outward nose at all, only noses for nostrils, the exquisite shape of the ear—which we justly consider unsightly and useless as regards ourselves—really attracts the attention as an object of beauty. The large males, although weighing twice as much as a full-grown man, have no larger genitals than a human child of tender age.

The largest maias I have killed measured 8 feet 10 inches in the stretch, being 12 inches longer than Mr. Wallace's largest. This monster was in the prime of life, having his teeth and nails unimpaired. The breadth of his face, including callosities, was 13 inches. I sent the skin and skeleton to Mr. Higgins, of Bloomsbury Street.

The infant maias, when taken, at once accommodates itself to circumstances, showing no fear of its captor, but looking to him quite naturally as its protector and feeder. If cold it will at once use a handkerchief, or even a piece of newspaper, as a cloak.

In the "Life of Apollonius" we read that in the distant East there is a country where pepper was cultivated, and the farmer employed apes to cultivate the vines. Now pepper has been from the most ancient times cultivated in Sumatra and Borneo, and the maias is found in each of these countries. The same work, however, makes mention of an exceedingly high mountain. Although we have Mount Ophir in Sumatra, it is not very remarkable, as it is *apparently* only the highest part of a long range. But in Borneo we have Kinabalu, much higher than Ophir, and, from the sea, apparently a detached mountain, and so more likely to gain notice as a high mountain. This Kinabalu is no doubt the mountain said to have been in the fortunate island to which the Chinese Emperors Che Hwang-*Ze* (which I take

to mean the Junior Lord of All Things) and afterwards Woo, who flourished about one hundred years before our era, sent expeditions in search of the Water of Life (see "Taonism," p. 238). And Kinabalu means simply the Chinese Widow.

Now I made enquiries of people, Muruts and Dusuns, who had domesticated the maias, and they said they had educated them so far as that they followed their masters to their daily labour at their farms, but never could be got to work. The most that was ever got from them in return for their food was that one, more intelligent than the many, had been taught to bring fire for his master's cigarette when requested to do so.

I should like to be able to inform you as to the period of gestation of the maias, and other points of its economy, but am not able to do this.

Note here:—I read a remark in your Journal that, notwithstanding (!) the fact that maias are not Carnivora, they have well-developed eye-teeth. The writer did not consider how useful these teeth are to the animal in tearing off the thick and rigid rinds of most of the forest fruit.

Note again:—One of your correspondents observes, as it appears in an extract quoted, that to the Dyaks of Borneo the maias is simply the orang utan, or "man of the woods." As a matter of fact, however, the Dyaks call the maias nothing else but "maias." In the north the Muruts call him "Kagui," and the Dusuns "Kaigiu." The only people in Borneo whom I know to call him "orang utan" are a single family of Arabs in the north, and very likely they got the name from the English settlers at Balambangan, at the close of the last century.

ANALYSES OF BOOKS.

Physiological Cruelty or Fact v. *Fancy: An Enquiry into the Vivisection Question.* By PHILANTHROPOS. London: Tinsley Brothers.

FIRST NOTICE.

BESTIARIANISM, like certain other parasites, is exceedingly tenacious of life, and is in no haste to quit its congenial home in certain human heads. Hence the work before us can in no wise be regarded as superfluous. On the contrary, from the moderation of its views and of its language, from the more than candour which it displays to opponents, and from the novel starting-point which it takes, it will be of great service in quarters where a sterner and more uncompromising advocacy of the rights of science might not find listeners. Our own opinion has repeatedly been expressed in the "Journal of Science:" it is that the "Vivisection Question" is utterly illegitimate unless raised by such persons only as refrain from inflicting pain and death upon animals for any purpose whatever. Such persons might, from their own point of view, consistently denounce physiological experimentation. But nature would effectually dispose at once of their arguments by "improving them off the face of the earth."

Our author, it will be found, calmly and quietly, but not the less cogently, exposes much of those exaggerations and misrepresentations which form the very staple of the so-called "anti-vivisection" movement. By means of such misstatements widely and industriously circulated, a strong prejudice against physiological research has been created, and many well-meaning men, and still more women, have been led astray. To remove the impression thus produced must be a work of time. Agitators of all grades well know that a falsehood may be conveyed in a few words, whilst its exposal demands considerable space. To this circumstance they owe their very existence.

"Philanthropos" at the very outset raises the question, "what is pain?" As a matter of fact we know nothing about any pain except what we have ourselves suffered. We infer that other men feel approximately as do we. But even here there is great uncertainty. It may be considered decided that most savage races, *e.g.*, the Red Indians of the western hemisphere, feel very much less than do we. When we come to the lower animals, our knowledge is very much more vague. We have so-called "signs of pain," that is, the starts, jerks, and struggles which wounded animals exhibit. But before jumping to the conclusion

that these movements are a proof of pain and of consciousness, we must consider that such "signs of pain" can be produced not only after accidents, which place a limb out of connection with the nervous centres, but even after death. What, then, can they prove? Our author cites the following portion of the evidence of Dr. Anthony before the Royal Commission on vivisection (Question 2,484):—"A horse can kick after he is dead. A knacker, after he has cut off a horse's head, will pass a long rod down the spinal marrow to destroy its activity; otherwise, as soon as he began to skin the body the legs would kick with sufficient force to break his arm." The author adds, "Such a motion in response to a stimulation is called in scientific language a reaction; in agitationist language, 'a spasm of agony.'" This phrase seems to have been invented by Miss F. P. Cobbe, who in the "Fortnightly Review" for January, 1882, wrote: "The very fact that physiologists select petted dogs to exhibit reactions (*anglice* spasms of agony) under their operations, &c." Without seeking why two Greek works should be given as the *English* version of so simple a term as "reaction," we would ask whether Miss Cobbe is really ignorant that such movements are no proof of pain? If so she is no very safe authority.

Philanthropos aptly shows that Bestiarians themselves, in their accentuated protests against the liability of cats and dogs to physiological experiment, admit that different animals do not feel pain equally, the more highly organised being more sensitive than the lower. How, then, can they maintain that brutes feel just as we do?

He shows also that in the physical sufferings of human beings —adults at least—there is a mental factor almost entirely wanting in animals. This something is ably described in a passage borrowed from Mr. E. Gurney and taken from the "Fortnightly Review" of December, 1881.

We read that "dogs kept at physiological laboratories for the purpose of experiments display no such horror of the operating-room as was described in a sensational paragraph which was sent the round of the papers some time ago. A dog released from experiment has been known to jump up upon the operating table and sit there to inspect a companion having his turn, showing that no very painful impression could have been left upon his mind by whatever he had undergone. Dogs with gastric fistulæ enjoy life in perfect health, and will sit up or lie down when told to have different substances injected or extracted. At Professor Ludwig's laboratory, in Leipzig, the dogs used as subjects for many such experiments are neither tied nor chloroformed, but merely patted and talked to while the process is going on."

"Among human creatures we see the effect of mental development upon the sense of pain very clearly in the case of children. An infant can be vaccinated without making it cry if its mind is kept occupied by a bit of sugar held before it; and it will undergo

even much more pain without discomposure if well amused. But when that child is three or four years older, he will understand that something is going to be done to him; he will be terrified at the preparations; neither sugar nor anything else will divert his mind, and he will be conscious of all the pain given, and probably exaggerate it from terror. If pain can thus be a secondary thought in the minds of infants it will be still more so in those of animals.

"A house-dog met with an accident by which a large piece of the skin and flesh above the eyebrow was cut and hung loose over the eye. His master, a surgeon (who furnishes the anecdote), determined to stitch it. Now, it is well known that—the skin being extremely sensitive—stitching is one of the most painful parts even of serious operations. The dog was taken into a shed, muzzled, and the cut stitched up. All the time that it was being done he was straining and struggling to get away, though never whining nor crying. The instant he was released he dashed into a corner of the shed and seized a bone which he had had his eye upon, and which had possessed his soul while he had been undergoing operation without anæsthetics.

"A horse, whose leg was badly broken, was sentenced to be shot; but there was considerable delay before the execution could take place. The bones were completely broken through, so that the leg hung loose, a state of things during which the least motion causes a human patient most exquisite agony. No suffering is worse than that from a broken bone, and the only way to prevent its becoming intolerable is to avoid the slighest jar which can grate the fragments against each other or the surrounding flesh. But during the two hours between its injury and its death this horse grazed and limped about to graze, carrying the broken limb dangling."

These two anecdotes, we learn, are vouched for by eye-witnesses, and we earnestly commend them to the consideration of all such "anti-vivisectionists" as are capable of dispassionate reasoning. The whole of this second chapter of the work before us will well repay attentive study.

The third chapter deals with the more difficult question, "What is cuelty?" The author's reply is, "the wanton or excessive infliction of pain." To this definition it may be objected that according to the foregoing chapter an estimation of the degrees of pain is exceedingly difficult. We should be inclined to call the infliction of pain and death cruel:

a. When objectless, or when the object to be obtained is frivolous.

b. When the inflictor feels a gratification in the infliction.

c. When the object can be obtained in some other manner without such infliction.

Our views differ here from those of Philanthropos mainly in that his defence seems merely to include experiments undertaken

from a medical point of view. There are, as we have shown on former occasions, many physiological investigations urgently needed, but which conceivably may never have any direct bearing upon the methods of the physician, the surgeon, or the veterinarian. In summing up this section the author writes :—"I cannot expect that the facts and arguments of the foregoing chapters will convince every one. When all has been explained and argued there will still remain irreconcileables determined to persist in their war against scientific experiments. But we can now see clearly what this war amounts to. It is an effort to keep many animals in suffering instead of few, men instead of beasts, the more sensitive creature instead of the less sensitive." To which we add that it is an effort, and in many cases a wilful, deliberate attempt to keep mankind in ignorance.

We now come to a discussion of that phase of the subject which has elicited from Bestiarians the greatest amount of perversity and wrong-headedness, to wit, the consideration of "Our Rights over Animals." It is somewhat strange that this question should first be prominently raised in an age which has recognised the struggle for existence more distinctly than has ever been done before, and has found what this struggle involves. Nature seems to have invested every organism with the right to destroy, devour, utilise at pleasure any other organism so far as it is able. We can only exist by exercising this right. If we become too weak, or too fantastically scrupulous, we sign our own racial death-warrant. The author writes :—"The opponents of such experiments assert that they are wrong in themselves, and that, therefore, it is time wasted to prove that advantages are gained by them, since no profit can justify a crime. I most unreservedly admit that no hygienic gain is worth a moral loss, and that the health of one man's body is too dearly purchased by the disease of another man's soul."

It seems to us that this admission is of the widest. It may strike the careful inquirer that a moral code which involves hygienic loss to the community is not improbably tainted with error. Perhaps, too, it might be added that grandmotherly legislation should allow the experimentalist to judge for himself as to the effects of his researches upon his soul.

The reasons given by Bestiarians why physiological experiments are wrong in themselves are summed up by the author under four heads :—First, that they are cruel ; second, that the infliction of pain hardens and demoralises the inflictor ; third, that it is unjust to make animals suffer for our good ; fourth, that it is an infringement of their rights which we have no authority to make."

The first of these contentions the author dismisses, like ourselves, as a *petitio principii*. Such experiments are not cruel from any rational definition of cruelty.

The second consideration holds good only when the production

of pain is the purpose of the inflictor, and when he finds pleasure therein. As applied to physiological investigations, it is simply an untruth, as we are able to assert from long and extensive observation. Work upon animals, conducted in a true scientific spirit, is no more hardening and demoralising than is experimentation upon plants or minerals.

As to the third position of the enemy, Philanthropos profoundly remarks that it merges into the wider question of vicarious suffering. "Is it right that one creature should suffer for the good of another?" That such should be the case seems to be "one of the most comprehensive laws of the sentient world." In all our ordinary dealings with animals we "constitute ourselves administrators of this law, and apply it to them for our own interests." As we showed ("Journal of Science," 1876, p. 318) and as Philanthropos here contends, we torment and immolate animals without scruple for our convenience, our luxuries, and our pleasures. It is only when pain is inflicted in the pursuit of knowledge that the Bestiarian conscience is exercised. The fourth contention—that "physiological experimentation is a violation of the rights of animals which we have no authority to commit," seems due to one of the bitterest of the "Anti-vivisectionist" fraternity—the editor of the "Spectator." This argument, which after all is a mere exaggerated form of the foregoing, is not merely demolished but annihilated by the principle of consistency. If the "golden rule"—which fails completely if we seek to apply it to criminals—forbids us to experiment upon animals, no less does it forbid us to hunt them, shoot them, eat them, castrate them, or make them work for our benefit or pleasure! We are told to "put ourselves in the place of the lower animals, and ask what we, with their pains and their sensitiveness, and their prospects of life and pain and happiness, might fairly expect of beings of much greater power but of common susceptibilities." Suppose any advocate of the "rights of animals" were troubled with Entozoa, would he reflect on such "rights" before taking anthelmintic medicines? If, then, he is willing to slaughter animals—possibly with pain to themselves—for the preservation of his own health, should he seek to restrain investigations which are calculated to preserve the health of his fellow men?

(To be continued in our next.)

Light Science for Leisure Hours. Third Series. Familiar Essays on Scientific Subjects, Natural Phenomena, &c. By R. A. Proctor, B.A. Camb., Honorary Secretary of the Royal Astronomical Society, &c. London: Longmans and Co.

We have here a collection of essays and reviews, reprinted from various journals. Their subjects are, as might be expected, for the most part astronomical, and among these six of the most important are devoted to solar eclipses and their lessons. "The Earth in Meteoric Shadow" we have already noticed on occasion of its first appearance in "Longman's Magazine." The paper on "Lottery Schemes" is selected as here admissible on account of the mathematical considerations involved.

"Conduct and Duty" is a brief critique on Mr. Leslie Stephen's "Science of Ethics." Mr. Proctor contrasts the point of view taken by Mr. Herbert Spencer with that adopted by Mr. Leslie Stephen, and expounds the criterion of the latter ethicist.

"Under the title 'Vivisection,' we find a short and favourable notice of Professor Owen's "Experimental Physiology." Mr. Proctor, though he occasionally writes for the "Spectator" and the "Echo," does not prophesy smooth things to the Bestiarians, and will accordingly be from time to time treated to missiles similar to those with which we are saluted :—

"Jam fæces saxaque volant."

"The American Tariff" is a subject upon which we cannot legitimately enter. At the same time, we cannot but think it hard that the American savant and student should be called upon to pay import duties upon foreign scientific books, upon geological, or biological specimens and apparatus, and upon apparatus.

The "Origin of our Race" has a somewhat equivocal title. We expected to find a disquisition on the origin of mankind; but the "race" in question is, it appears, that sub-section of humanity which now occupies Britain. Concerning this race the author takes the very rational view that complexion, type of features, and shape of head form a more trustworthy evidence than language, and that the views of certain historians, based upon traditions and upon linguistic points, must be rejected. That our language is essentially Teutonic no one will care to dispute; but that in blood we belong very largely to a dark race seems even yet more plain. The only escape from this conclusion is the theory that as any nation advances in culture its complexion takes a more southern type. Some contend that this process is no less marked in France than in Britain. It must be remembered that, according to Roman authorities, the Gauls were no more a dark-haired people than were the ancient Germans.

"A Late Boat Race" does not fall within our cognisance.

"Are we Jews?" is, again, a paper with an equivocal title. Seeing its title in the contents, we expected a discussion on our alleged "Israelitish Origin." We find, on the contrary, an essay on Sabbatarianism, with reference to the refusal of the inhabitants of St. Kilda to land provisions sent for their relief on a Saturday lest the work might not be completed by midnight. The author further denounces the discontinuance of the weather warnings sent to the Continent from our Meteorological Office, as far as the Sunday is concerned. In so doing he has abundant justification. But we cannot go with him in the following contention:—"Sabbatarians lay great stress on the assumed fact that the rest is found good for body and brain—a fact which, if proved, would mean little more than that long-continued habit has made such rest a necessity. But they pay little attention to the fact that nature knows no seventh day's rest. The earth does not pause in her orbital motion round the sun, nor the moon on her motion round the earth. The tides and currents of the ocean continue their motion, and the waves rest no more on the Sabbath than on week-days. Winds blow and rains fall on that day as on the rest. All forms of life, vegetal and animal, continue unaffected, save only as they are related to man in those countries where the seventh day's rest is observed!"

To this strange argument we may reply that the comparison here drawn from the inanimate world is utterly beside the question, and if it could prove anything at all, it would show that our nightly rest had been made a necessity merely by long-continued habit. Plants have, under various forms, intermissions of rest and activity. Wild animals are nowhere goaded into unceasing toil from the beginning to the end of the year. Hence, for them, a seventh day's rest is not required. As to man, he has never, in the savage or in the barbarous state, or in the ancient civilisations, been driven to work as at present. Of this overwork, the consequences are only too patent. So far, then, from fancying that a day of rest is a mere matter of habit which might be dispensed with, we hold that our leisure wants increasing. If the easy-going shepherds and herdsmen of Syria needed one day's rest in seven, we require more. What name we give our rest, whether it should come in the form of entire days, or of portions of days, or whether an entire community should desist from its toil at once, are secondary questions with which we cannot meddle. We presume Mr. Proctor wrote this essay before the appearance of Mr. Herbert Spencer's memorable farewell address to the American people.

"Paradoxes and Paradoxists," though written eleven years ago by occasion of De Morgan's "Budget of Paradoxes," is still worth careful reading. The criterion given for distinguishing the mere paradoxist from the great discoverer will, we believe, in most cases, be found to work satisfactorily. "It is a thing

which has never happened for a man without wide and thorough knowledge of a branch of science to make great discoveries in that particular branch." Of course an ignorant man may by hap observe some important fact hitherto unrecognised, but the probability is that if such a fact come before him he will see neither its importance, its meaning, nor its novelty. The vulgar contention "that it is unlikely for some unknown person to succeed where Newton, &c., have failed," is dismissed with the contempt it merits. Perhaps the most offensive instance of this fallacy in existence is to be found in a pamphlet by the Rev. F. O. Morris, B.A., entitled "All the Articles of the Darwin Faith" (see "Journal of Science," 1878, p. 467).

The advocates of the so-called "Zetetic Astronomy," are naturally weighed in the balance, and found wanting. Among them figures, of course, "Parallax," known in 1849 as S. Goulden, and afterwards as Rowbotham. Mr. Proctor happily remarks that the name parallax "may have been chosen to indicate his wish to change the accepted views of the universe, for does not Liddell and Scott's 'Lexicon' give as the meaning of the verb *Parallasso*, '*to change or alter a little*, especially *for the worse*,' or else, as indicative of the ingenuity of the man in avoiding any argument which appeared irresistible (for the verb means also to *slip aside*, *escape*, or *dodge*). His skill in this respect was remarkable. He would reason at any length with those who had a smattering of astronomical knowledge, and he would only occasionally, by some reference to scripture, exchange what resembled argument for an 'appeal to the gallery'; but when a really unanswerable argument was brought against him, he had a simple but effective formula, which always afforded a loophole for escape: 'Come, sir,' he would reply, 'you have said your say; others wish to speak as well as you.'" So much for public discussions!

A smile will, perhaps, be called up to the face, and an old adage to the memory of the reader when he encounters the following passage:—"No one is likely to believe that there is a semi-transparent moon merely because 'Parallax' says there is; while probably nine-tenths of those who have read Lockyer's 'Elementary Lessons in Astronomy' are satisfied that seamen find local time 'by observing when the sun is at the highest point of its path' (article 564); that when the sun moves fastest the dial will get before the clock, and *vice versâ* (article 416); and so on with the other blunders in that too clever compilation. But, though the mischief done by the paradoxist with his false science is less serious than that accomplished by those who write about real science without sufficient knowledge, yet the study of paradoxes is not on that account to be recommended."

An essay on the "Influence of Marriage on Death and Crime," opens with the remark:—"There are reasons for believing that a good hearty blunder has a better chance of thriving than an

average truth." The author writes:—"I take it that of all possible resources for those of tainted or feeble constitution, of strongly criminal tastes, or of insane tendencies, matrimony is, on the whole, the most objectionable, as far as the community at large is concerned. If a consumptive person could add ten years to his life by marrying, he might also add ten consumptive persons to the next generation; and who shall say how widely the taint would spread in later years? The criminal is hardly likely to be influenced by other than selfish considerations; but most certainly the community has no reason to desire that he should be encouraged to increase and multiply criminality. As for a person who inherits a tendency to insanity and knows it, such a one, if he entertains whilst still sane the thought of marrying, is a criminal of deepest dye, and the worst thing that can happen to the community is that his insanity should delay to manifest itself until the taint has spread to his innocent offspring.

"Light Science" will supply many persons with matter for useful reflection.

Longman's Magazine. No. VI. April, 1883.

In this issue we have to notice a paper by Mr. J. K. Laughton, entitled "Wind-force, and how it is Measured." The author chronicles the effects of some of the greatest storms on record, those especially of November 26th, 1703, and of October 14th, 1881. He suggests the damage done to trees as furnishing a standard for roughly comparing the force of these visitations. It is, however, not overlooked that the storm of 1881, though far more destructive than that of 1703, came at a season when the trees were still in leaf, and when the ground was softened with rain. Furthermore, the wind came from the N.N.E.—an unusual direction for an autumnal storm.

There is another consideration of some moment. The entire area of woodland now standing in England is very much smaller than was the case in 1703, so that the destruction of (say) 40,000 trees in our day shows a relatively greater devastation than the overthrow of a similar number a couple of centuries ago. The exorbitant degree to which the surface of England has been denuded of trees gives also fuller play to storms than was formerly the case. Hence we find it here remarked that though the damage on land was greater in 1881 than in 1703, the havoc at sea was smaller.

Mr. Laughton explains the difficulty of measuring with any approach to accuracy the velocity of, or the pressure exerted by,

the wind in storms. Taking the best instrument for the purpose, which is probably Robinson's anemometer, now employed in this country, over the greater part of Europe, and in America, the question arises—Where is it to be fixed up ? We want a situation where the wind is neither intercepted nor diverted from its even course. Everyone must have observed that when wind blows against a wall it rebounds in all directions, and rushes with a concentrated force round the end or over the top of the obstruction. It is observed by gardeners that these deflected currents are far more ruinous to vegetation than the direct sweep of the wind in the open. It is at the corners of streets that umbrellas are turned inside out and that hats are whirled off into the realms of space. Hence the only spot at all suitable for erecting a wind-gauge is at the top of a pole, 20, 30, or perhaps 40 feet high, set in the midst of an open field. We may add the further condition that this field should be in a level country. If on the slope of a hill, or amongst hills, the force of the wind may be either partially broken or intensified, according to its direction.

There is a further consideration : we have no reason to suppose that the velocity of the wind in a tempest is uniform from minute to minute or from half-hour to half-hour. Even where there is no scope for interception, deflection, or rebounding, as on the open sea, we observe gusts of intensified violence intervening between lulls. Hence the average hourly velocity of the wind, however fairly measured, does not tell us what has been its maximum velocity. Now this maximum velocity is precisely what the engineer needs to know in order to adapt his structures to the greatest pressure which they may probably have to encounter. This maximum velocity we are hitherto unable to find with anything like precision, and the highest authorites are here not at one. Our ignorance on this point was brought into full relief on occasion of the Tay Bridge disaster. As Mr. Laughton puts it, " Engineers of the highest repute had no exact knowledge as to whether the extreme probable force of wind should be taken as ten pounds on a square foot, or twenty, or forty."

In the tempest of October 14th, 1881, a pressure of 53 lbs. per square foot was indicated at Greenwich ; and at Liverpool, in March, 1871, a pressure of 90 lbs. is said to have been registered. This result has been strongly questioned as far as England is concerned. But the speed of an American tornado is estimated as having a mean velocity of 392 miles per hour, and a maximum of 800 miles, which answers to a pressure of three-quarters of a ton per square foot. Such figures, to those who judge merely from the ordinary power of the wind, seem absurd. But if we examine the recorded effects of such tempests we find that the question takes a different aspect. In the tornadoes of the Western States heavy pieces of machinery have been carried away to considerable distances. In a West Indian hurricane heavy artillery has been dismounted and hurled away.

In speaking of the devastation of the town of Walmer by the whirlwind of October 24th, 1878, Mr. Laughton agrees with Mr. Symons in the opinion that much of the damage in such cases may be due to a sudden rarefaction of the air. A rarefaction to the extent of 0·2 inch of the barometer would give the air in buildings over which it passes a lifting power of about 14 lbs. per square foot. Hence in the Walmer storm we have record of "the vertical lifting of slated roofs, and the simultaneous *outward* explosion of the walls, even *towards* the wind." A whirlwind that passed near Vendôme, October 3rd, 1871, took off the roof of a barn, and lifted bodily out of it a cart which had been left inside. "The walls of the barn were not injured, but the cart was found 150 yards away."

We must here remember that the difference between a tempest —or even an ordinary wind—and an explosion is merely one of degree. Wind is gaseous or aëriform matter in motion. So is explosion, the motion being in this case the sudden conversion of matter from the solid or liquid to the aëriform state, which of course implies vast expansion. Explosion shatters bodies in pieces, because the velocity of the gaseous matter is so great that solids exposed to it have, in familiar language, no time to yield to the pressure.

The Mason College Magazine. Vol. I., No. 4, April, 1883. Birmingham: Cornish Bros.

THIS little periodical contains more thought, and more matter provocative of thought, than some of its bulkier and more widely-circulated contemporaries.

First in place, as perhaps in importance, is a paper by Miss Constance C. W. Naden, discussing the question "Is the increasing predominance of Brain over Muscle conducive to National Welfare?" The problem to be solved is one of extreme difficulty. As far as our observation goes bodily vigour and endurance form a necessary substratum for continued cerebral activity. It must be conceded that brain-work under the pressure of anxiety is fearfully enfeebling; but we cannot see that the scientific specialist of the present day is necessarily working under more unhealthy conditions than the polyhistor of the past. The very contrary is the case; the geologist or the biologist, if of the modern school, is bound to spend much of his time in the open air, instead of sitting at a library table poring over what other people have thought and said before him. Consequently he retains his physical vigour in much higher degree than many people who are not fairly classed among

brain-workers. Again, even the most exclusive specialist, be he entomologist, fungologist, muscologist, or the like, has, as a rule, a passionate love for the beautiful in Nature far more intense than is found in average mortals. So that whilst his intellectual life is quickened, his æsthetic being also is maintained in activity.

But we can fully admit that we as a nation, though we do less brain-work than one at least of our neighbours, expend more brain-power, in consequence of our unhappy system of competitive examinationism. Here lies the national danger. The champions of this system should be called upon to show why it will not, in our case, result in the same mental stagnation which has resulted from it in China. We can never bear too clearly in mind that all the world's best brain-work is non-competitive.

Mr. Greatheed, in his reply to Miss Naden, brings forward some points which ought not to be overlooked.

"An Unreported Debate" is a strange production. We have the "Apostle of Sociology" moving a resolution "that man is a degenerated frog," whilst the negative and affirmative sides of the question are upheld by the "Man of Character," Hypatia, Demosthenes, "1001 Gems," "The Bookworm," the "Queen of Hearts," and others, the motion being ultimately carried by a large majority.

An interesting debate on the difference between Reason and Instinct is somewhat briefly reported, and by a misprint we read "that the difference between Reason and Intellect is one of degree."

We are happy to find that the Physiological Department of the Mason College is in an active condition, and that the instruction given is of a thoroughly practical character.

The Popular Science Monthly. Vol. XXII., No. VI. April, 1883.

In the issue before us of this valuable organ of scientific opinion there are certain papers which justify an especial notice.

Mr. McElroy's remarkable memoir on the "Economic Function of Vice" is, in one of its phases, discussed in an article in our current number. We cannot help, however, noticing with regret that the writer seems to recognise but one motive for effort, that is to say the "passion for worldly advancement." He writes: "The humblest work incessantly to lift themselves into the ranks of the middle classes. The middle classes strive as earnestly to make themselves plutocrats, aristocrats, and lordlings. When a man at last reaches the social summit he desists

from further efforts at improvement, or if this period (? point) comes too late in his life his children do it after him. Some vice speedily removes him, and clears the ground for another man to come to the front, who is also removed summarily when he becomes obstructive." This, if true, would be a very sad picture of the world. The best and noblest work has little, if any, direct influence on the social position of those who execute it. There are, we are thankful to say, men not a few who, if only in such a position that their time is at their own disposal, entertain worthier aims than becoming "plutocrats or aristocrats." Such has been the career of the Darwin family since the time of the Great Rebellion. We devoutly hope, if the world grows wiser, that wealth and title will be less exclusively worshipped, and, in consequence, less generally sought for than they are at present.

Dr. F. L. Oswald completes his suggestive dissertation on the "Curiosities of Superstition." He makes the significant and perfectly true remark that "mental aberrations can become epidemic." The writer sarcastically declares that: "The precipitate follower of Bacon has noticed the coincidence of cold weather and catarrhs, and jumps to the conclusion that a low temperature deranges the functions of the respiratory organs." Might we remind him that in the severest Siberian cold respiration is so much interfered with that every breath is painful? On the subject of alleged forebodings we find a startling utterance. "Apollonius of Tyana foresaw even the great *aphanasia*, the fifteen hundred years' eclipse of common sense and reason. 'Woe be to our children!' he exclaimed on awakening from a trance; 'I see a shadow approaching; a great darkness is going to cover this world.'" If these words were really uttered they seem a strange prediction of the decay of the ancient classical civilisation, of the relapse into barbarism, and of the dirt, the asceticism, and the unreason of the Dark Ages.

Dr. W. A. Hammond communicates a paper on "Perceptional Insanities"—hallucinations far more common than is generally supposed.

Certain "Speculations on the Nature of Matter," by Mr. H. H. Bates, will be reserved for future consideration.

Under the heading "Editor's Table" some noteworthy remarks are made on the new weekly American journal which has recently appeared. Whether it is to be regarded as a resuscitation of a former journal bearing the same name, which we used to read with both pleasure and profit, we do not find stated. We note the plan of our new contemporary and the important functions which it is to perform, which, however, will *not* include advocating the interests of a "Science and Art Department." Its enunciations on "The Future of American Science" seem to us, however, as well as to the Editor of the "Popular Science Monthly," open to criticism. That future is proclaimed to be

distinctly and super-eminently utilitarian. We shall regret should such be the case. Strictly speaking there is no "utilitarian" or "applied" Science. What commonly bears the name is industrial art. The writer admits that "the man who discovers nothing for himself, but only applies to useful purposes the principles which others have discovered, stands upon a lower plane than the investigator." Yet he continues:—"But when the investigator becomes himself the utiliser, when the same mind that made the discovery contrives also the machine by which it is applied to useful purposes, the combined achievement must be ranked as superior to either of its separate results." No, we reply, a thousand times no. The highest discoveries are incapable of direct utilisation. What "machine" can apply to useful purposes the life-work of Darwin, the periodic system of Mendelejeff, the atomic theory, the doctrine of gravitation, the discovery of the circulation of the blood, the researches of Joule, of Lyell, and, in fact, all the great "epoch-maching" triumphs of research? It is only the smaller discoveries, the bye-issues, which fall, as it were, crumb-like from the table of Science, that can take the form of direct inventions.

We must further consider that if the investigator forsakes his own task to devote himself to inventions and patents, he sins against the great principle of the division of labour.

The writer in "Science" touches closely upon the ridiculous when he writes: "While the scientific cynic may condemn the utilitarianism of our age, the more liberal man rejoices in it." Surely we have here a gross perversion of language. The man who rejects selfish considerations cannot rationally rank as a "cynic," whilst he whose motive is greed can scarcely claim to be called, in any honourable sense of the term, "liberal." We hope that this ill-advised essay does not strike the key-note of "Science."

Sound. By JOHN TYNDALL, D.C.L., LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain. Fourth Edition. London: Longmans and Co.

THIS work, which has already reached its fourth edition, is characterised by its clearness—we might say elegance—of exposition, and by the total absence of mathematical formulæ. To take an illustration, the reader finds here the completed building, free from the ladders and scaffolding used in its construction. Hence it is intelligible not merely to the physical specialist, but to a much wider circle. This is doubtless the author's purpose

in entitling his book "Sound," instead of "A Treatise on Acoustics." It would be a great mistake, however, to suppose that we have here a popular treatise in the common and objectionable sense of the term. We do not find here inaccuracy, looseness, or the curtailing and perhaps omission of important truths. The production and propagation of sound, and all its essential phenomena, are explained in a manner intelligible to common sense.

Among the facts not generally known, which may here be met with, is Hooke's anticipation of the stethoscope, which the author well puts forward as an instance of the action of scientific imagination.

In drawing a distinction between noise and sound, Professor Tyndall advances an opinion which is not altogether admissible. He writes:—"The action of noise upon the ear has been well compared to that of a flickering light upon the eye, both being painful through the sudden and abrupt changes which they impose upon their respective nerves." That noise is often painful needs no demonstration, but it is painful not as such, but in virtue of loudness and accentuation. Few noises are more distressing than, *e.g.*, the tolling of a bell. Here the sound, however musical, is loud, sharply marked, and is repeated at regular intervals.

Several important books which have been received for review must stand over from want of space.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

DO SNAKES SHELTER THEIR YOUNG IN THEIR THROATS?

To the Editor of the Journal of Science.

SIR,—May I intrude on your space to reply briefly to the letter of Mr. James Simson, of New York, in the May number of your Journal, with reference to the question "Do some snakes offer a refuge to their young, in their own throats?"

First, I would thank Mr. Simson for sending me his interesting work, duly forwarded by Messrs. Griffith and Farran. I must also, under present circumstances, express regret at my omission of Mr. Simson's painstaking investigations on this interesting question in my work on "Snakes." Truth to tell I did see the English edition of Simson's "Contributions to Natural History" soon after its publication in 1875, and even quoted it in the first sketch of my 27th chapter; but I found that references to it must partake too much of the nature of a controversy (Mr. Simson will pardon me for not coinciding entirely with his ideas), and that important quotations from the "Reports of the American Convention on Snakes," held in 1872, as well as those from our English zoological journals, already occupied more than the space allotted to other chapters. My task was indeed no easy one, some deference being due to certain able zoologists in England who doubt the existence of any maternal affection in reptiles. By this time Mr. Simson has, I trust, been able to look at my work, which can be obtained from Messrs. Dutton and Co., of New York, and he will therein discover that "a careful examination of the anatomy of the snake, to ascertain the physical peculiarities connected with the phenomenon described," has been made again and again; and that there was no indication of the "two throats in the female snake," as suggested by Mr. Simson, but that there seemed every possibility of ample refuge in the expansile œsophagus. The young are supposed to go no further.

Mr. F. W. Putnam, before writing his valuable paper in the "American Naturalist" (1869, quoted by Prof. Browne Goode at the American Convention), had made himself acquainted with these anatomical investigations, conducted by Dr. Edwardes Crispe, of London, so long ago as 1855, and published in the Zoological Society's Transactions subsequently, together with further corroborative testimony. All this was fully discussed at the Convention, the "Reports" of which do not appear to have fallen under Mr. Simson's notice, but which would put him in possession of all the evidence collected up to that date.

"I lay it down as an axiom that we must hold that *all* snakes swallow their young, till the opposite can be proved of any particular species of them," says Mr. Simson, page 29 of his "Contributions"; and again, on page 198 of his Appendix (American edition):—"To prove the contrary" would be difficult,—nay impossible, unless every female snake on earth were watched from birth to death! Besides, are we warranted so to establish a fact in Natural History? Mr. Simson will see that I have ventured to put forth some speculations of my own, restricted to certain species, and founded on personal observations (page 500 of my work).

With regard to snakes in captivity, we certainly have some evidence of maternal instincts in a display of unusual irritability and spitefulness during the period of incubation. And it is now a recognised fact that several species of snakes, formerly supposed to be entirely indifferent concerning their eggs, keep, on the contrary, a careful watch over them, or even incubate them in a nest of their own contrivance. The still greater phenomenon of the retarded deposition of eggs, or of an oviparous snake becoming a viviparous one, would seem to be due to maternal solicitude, and to have some bearing on the question of the maternal refuge (p. 499 *et seq.* of "Snakes").

With all respect for Mr. Simson's zeal, we must still regard the question as a "vexed"—because, in the eyes of the authorities in England, an unproved—one. Our one native viper is now becoming scarce, and unprejudiced observations are rare. Prof. Browne Goode succeeded in gathering together an immense mass of evidence during the summer of 1872. Let the same class of observers in America contrive to secure without violence any female snake seen thus to refuge her young, and send her, *with her progeny in her throat*, to some of the scientific authorities. It would not be difficult to catch her; for those who would not venture to grasp a rattlesnake might, without hesitation, seize the head of one of the harmless kinds which are said to display this protective instinct, and twist a string so tightly round her neck that the young could not possibly escape.

In the case of a venomous serpent, a cool and courageous farmer—and there are many such in America—might adroitly throw a cloth or a handkerchief over its head, which held down

with the foot or a stick would prevent the escape of the young; a cord may then be tied tightly and speedily round the throat, *over* the handkerchief, effectually capturing the entire family, which have been seen to enter the mouth. Then plunge the whole—handkerchief, cord, and all—into a jar or bottle of alcohol, and dispatch it to Prof. Baird, of the Smithsonian Institute, or to some other American herpetologist. Better still, pack the bottle in a box of sawdust, and address it to the Zoological Society of London, 11, Hanover Square. They would, no doubt, be willing to pay a good price for so valuable a specimen, which, from a *well-accredited witness*, would "set the matter for ever at rest" in the Old World as in the New.

Unfortunately the tendency to fill up spare corners of newspapers with "snake stories" in America, where humorous journalists vie with each other in outdoing the marvellous, throws discredit on the few incidents that may be true among them; for—to use the words of Prof. Owen, when regretting this misuse of journalism—"it is far harder to kill an *untruth* than to establish a *truth*."

With snakes in captivity we are unlikely ever to witness this display of maternal refuge, since the cause of provocation cannot exist. It would be impossible to so entirely seclude the snakes that they should be unfamiliar with human beings. They must be fed and otherwise cared for, and so become, to a certain extent, "tame" in their abnormal condition.

Let me conclude with a hope that the present summer will not pass without the capture of at least one of the many pattern-mother snakes annually recorded as living refuges.—I am, &c.,

CATHERINE C. HOPLEY.

PALÆOZOIC ROCKS IN AMERICA.

To the Editor of the Journal of Science.

SIR,—Referring to your remarks on Mr. Kinahan's memoir on Palæozoic Rocks in Galway and elsewhere in Ireland, supposed to be Laurentian (Vol. v., No. cxi.), I observe you refer to Dr. T. Sterry Hunt, who "twenty years ago admitted that certain Donegal rocks could not be distinguished from altered palæozoic strata in America."

Dr. Hunt has changed his opinion, and no longer—since 1870 I think—regards these strata as altered palæozoic, but as probably Huronian.

I have myself carefully examined them since then from Vermont to Gaspé, and am in a position to assert and to prove that the whole evidence is in favour of their pre-Cambrian age.

Physically and mineralogically they correspond with the Huronian *series* of Sir William Logan, which some authors have asserted, without any sufficient evidence, rest unconformably on the Laurentian: this assertion Sir William Logan himself never ventured to make, and the truth of it no member of this Survey has yet been able to confirm, though many miles of both Series have been examined from Nova Scotia (Cape Breton) to Lake Winnipeg. The result has been, in the East, to cause us to give up the attempt to define them as separate formations, and to classify them all simply as pre-Cambrian. A similar result has, I understand, attended a similar attempt on the Pennsylvania Geological Survey. I may add that I have no doubt whatever of the correctness of recent views respecting the schists, &c., of Anglesea and Caernarvon, which I mapped on the British Survey with Sir A. C. Ramsay some thirty-five years ago, namely, that they are likewise of pre-Cambrian age. They correspond exactly with the so-called altered Quebec group of Logan; but here the evidence of stratigraphical position is much more distinct,—indeed it is so plain along a line of more than 150 miles as to leave no reasonable doubt on the matter.

Some day we may discover a sound basis of separation for Laurentian and Huronian, but as yet this has not been done in Canada, unless mere mineralogical differences in groups of beds are to be taken as such,—a feature which appears to be also characteristic of the supposed Laurentian rocks of Ireland.

The subject is one I take great interest in, and, though I have written but little about it, I have perhaps studied it over a wider geographical range than any other living geologist.—I am, &c.,

ALFRED R. L. SELWYN.

A BOLIVIAN SAURIAN.

To the Editor of the Journal of Science.

SIR,—The "Anglo-Brazilian Times," March 24th, 1883, says that "the Brazilian Minister at La Paz, Bolivia, has remitted to the Minister of Foreign Affairs in Rio photographs of drawings of an extraordinary Saurian killed on the Beni after receiving thirty-six balls. By order of the President of Bolivia the dried body, which had been preserved at Asuncion, was sent to La Paz. It is 12 metres long from snout to point of the tail, which latter

is flattened. Besides the anterior head, it has, 4 metres behind, two small but completely formed heads (?) rising from the back. All three have much resemblance to the head of a dog. The legs are short, and end in formidable claws. The legs, belly, and lower part of the throat appear defended by a kind of scale armour, and all the back is protected by a still thicker and double cuirass, starting from behind the ears of the anterior head, and continuing to the tail. The neck is long, and the belly large and almost dragging on the ground. Professor Gilveti, who examined the beast, thinks it is not a monster, but a member of a rare or almost lost species, as the Indians in some parts of Bolivia use small earthen vases of identical shape, and probably copied from Nature."

If this account should prove to be accurate it would form a counterpart to the etching of the mammoth which forms so interesting a memorial of pre-historic art.—I am, &c.,

WILLIAM E. A. AXON.

Fern Bank, Higher Broughton, Manchester.

NOTES.

BIRD-EATING FROG.—The following curious narrative is taken from the "Cape Times," March 27th, 1883:—"A lady living in the George district supplies the 'G. R. Herald' with the following particulars of the remarkable habits of this creature:—'I have much pleasure in furnishing all the information we have regarding the large frogs which have proved so destructive to our young chickens. A water-sluit runs round our terrace, and passes through the ground over which the poultry range, and in this the frogs harbour. The first time our attention was drawn to their bird-eating propensity was by the cries of a small bird in a fuchsia near the stream. Thinking it had been seized by a snake, several hastened to the spot, and saw a beautiful red and green sugar-bird in the mouth of a large greenish frog: only the bird's head was visible; and its cries becoming fainter, the frog was killed and the bird released. Its feathers were all wet and slimy, and for some days after we could distinguish it in the garden by its ruffled plumage. Since then the same species of frog has on several occasions been killed with young chickens, half-swallowed, and once a duckling was rescued from the same fate. Whether the noise is natural to these frogs, or assumed to decoy the chickens within their reach, we know not; but they constantly make a chuckling sound so exactly like a hen calling her chickens for food that we have seen whole broods deceived, and rushing towards the sluit where they supposed the hen to be. The frogs are very wary, and it is difficult to find them unless by the screams of their victims. We have lost large numbers of small chickens in an unaccountable manner, and feel sure now that these frogs must be answerable for very many of them, as there are no rats here, and the chickens are carefully housed at night. If I can give you any further details I shall be glad to do so.'"

Quinine and cinchonine, if introduced hypodermically into Guinea-pigs, rabbits, and dogs, prove fatal if the dose is sufficient. A dog weighing 12 kilos. died from a dose of 2 grms.

One Joseph A. Steiss, D.D., in a treatise on "Primeval Astronomy," declares Evolution "a lie." This is a simple and "primeval," but unfortunately not conclusive, style of argument.

Prof. Palmieri announces the existence in the lava of Vesuvius

of a substance giving the spectrum line of "Helium,"—an element hitherto recognised only in the sun.

Doubts are thrown ("American Journal of Science") by competent authorities as to the nature of the alleged "lignified serpent" of Matto Grosso. It is suggested that the specimen is merely the track of a wood-eating larva, which has been filled up by the growth of new wood.

M. C. Dareste ("Comptes Rendus") finds that eggs, if submitted to intense and numerous vibrations, almost invariably produce monstrosities. This teratogenic cause is the more remarkable as coming into play *before* the development of the embryo. Fecundated eggs which have been carried by railway prove frequently barren if placed under a hen immediately after a journey. If they are allowed to rest for about three days before incubation they give rise to chickens.

Mr. H. Spicer ("Light") gives an account of the spirit of a Pomeranian wolf-hound having appeared to its master at the moment of death.

Mr. W. F. Barrett, in the same journal, describes a clairvoyante as seeing with great difficulty when the wind is high and the air charged with electricity. When there is a snowstorm she cannot see at all.

M. Hervé Mangon, having observed that *Mesembryanthemum crystallinum* takes up from the soil an extraordinary quantity of alkaline salts, proposes to employ it for removing the excess of such salts from land on the sea-coast and in salty deserts, so as to make it gradually fit for ordinary vegetation.

According to "Forest and Stream" an albino raven is now living in the Berlin Aquarium.

M. J. Reiset ("Comptes Rendus") finds that the products of respiration in animals depend more on the nature of the diet than on the species. A part of the nitrogen set free in the transformation of the albumenoid matters is eliminated as gas. In herbivorous animals there is a considerable escape of a hydrocarbide.

M. A. Robin ("Revue Scientifique"), in an investigation of the anatomy of the Cheiroptera, shows that in different species the uterus varies from the simple form which it has in the higher Primates to a double organ with two separate openings into the vagina.

According to "Wiedemann's Beiblätter" a shark belonging to the genus *Scymnus* is phosphorescent over its whole under surface, with the exception of a black stripe on the neck. The upper surface is non-luminous.

It is being now generally admitted that the "origin of the fittest" is no less important than the "survival of the fittest."

According to Mr. D. Morris the mungus is the most efficient rat-catcher yet introduced into the West Indies. Unfortunately they do not confine their attention to rats.

According to Dr. Miklucho Maclay some of the native tribes of New Guinea do not even know how to kindle a fire.

Prof. A. E. Verril, in a paper reproduced by the "Scientific American," comments on the absence of vertebrate remains on the sea-bottom beneath the Gulf-stream.

We regret to learn, from the "Medical and Surgical Reporter" and the "New York Medical Journal" that an "Anti-Vivisection" Society is about to be formed in Philadelphia. Nobodies and charlatans will find this an excellent opportunity for gaining notoriety. Yet "Science," writing but a little earlier, says, "American Science has nothing to fear from the future."

Mr. F. W. True, of the U. S. National Museum ("Science"), calls attention to the swinging of the human arms when walking as an inheritance from a semi-quadrupedal state.

M. Ed. Perrier ("Comptes Rendus") concludes, from the results of deep-sea dredgings, that the abysmal fauna is in great part descended from that of the littoral regions, and has become acclimatised in the depths. The conditions of existence being there constant, this fauna exhibits little variation.

According to the "Indian Forester" Australian acacias, if transplanted to the north of India, after many years gradually modify their time of flowering to suit the change of climate, and blossom in June instead of October.

Mr. Jeffries describes, in "Science," a hermaphrodite bird, a green-tailed towhee (?), from Colorado. The sexual organs were male on the right side, and female on the left. The plumage was that of a female.

According to Mr. J. Jamieson (Royal Society of Victoria) a direct action of the sun's rays, as such, upon bacteria has not been definitely shown.

M. J. Reiset ("Comptes Rendus") shows that cow's milk, as it issues from the teat, is not alkaline, but rather inclining to acidity.

M. J. A. Fort ("Comptes Rendus") has made an experimental study of the physiological action of coffee. He finds that its immediate effect is to excite the central nervous system, and that it neither increases nor diminishes the proportion of azotised food which requires to be consumed.

According to the "Popular Science Monthly" a genuine aquarium devoted to biological research is about to be established by the United States Commission of Fish and Fisheries, at Wood's Hole, Massachusetts.

Herr Engelmann ("Botanische Zeitung") has demonstrated experimentally that only the pigmentiferous cells in plants, and in these only the pigmentiferous particles of plasma, are capable of giving off oxygen under the influence of light. In the vegetable world there exist, besides chlorophyll, a series of other colouring matters (chromophylls) which exert an assimilatory action.

Dr. J. D. Caton ("American Naturalist") gives instances of curious friendship between a sand-hill crane and a family of swine; also between a Hawaiian goose (*Bernicla sandwicensis*) and a litter of young pigs. A male Wapiti deer, in the Ottawa acclimatisation grounds, forsook his mate and attached himself to a Durham heifer, but his frequent intercourse with her did not lead to the production of hybrids.

E. Baumann ("Pflüger's Archiv.") criticises very sharply the results of Loew and Bokorny on the chemical cause of life. He maintains that chemical distinctions between living and dead protoplasm have long been recognised, and that the reaction recognised by L. and B. is not of general importance.

M. Cornu ("Naturforscher") proves that gases may be absorbed through the epidermis of the aërial organs of plants. The grapes in a vinery were spoiled because the gardener had coated some plant-stages with badly-smelling tar-oil. The offensive flavour was stronger in the centre of each grape than on the skin. The roots and stems of the vines were outside the house.

According to Col. Venukoff there exist great anomalies in the different magnetic elements in the neighbourhood of Belgord, in the province of Kursk. There is no trace of iron-ore in the district.

Prof. Bruns, of Tubingen, finds that a portion of marrow, freed from bone and inserted under the skin of the same animal at a point distant from its origin, gives rise to the formation of bone and cartilage at the point in question.

A new vegetable parasite, *Haplococcus reticulatus*, has been recently discovered in pork by Dr. Zopf. It occurs in from 30 to 40 per cent of the animals examined. Would it not be well if we paid more attention to the sanitary legislation of Moses, doubtless a fragment of the ancient medical lore of Egypt?

In Italy the water of hot springs is being extensively used in

horticulture for the production of early crops. The same thing is being done on a large scale in Japan.

L. Palmieri ("Rend. Accad. di Napoli") observes that if several chlorides are introduced simultaneously into the same flame, the spectral lines of only one of them are often seen. Copper chloride, unless in large excess, is not visible in presence of sodium chloride.

There has lately sprung up an Association, or Union, or "Movement," for opposing the "Deceased Wife's Sister's Agitation." Characteristically, there is among the members of the committee not a single medical man or physiologist—the only class entitled to be heard on the question.

MM. W. Wroblewski and Olszewski have solidified carbon disulphide at $-116°$ C., alcohol at $-130{\cdot}5°$ C., and have obtained oxygen as a colourless mobile liquid at $135{\cdot}8°$ C., under a pressure of 22 atmospheres.

During anti-cyclones the temperature of mountain stations fluctuates strangely within a few hours. According to "Ciel et Terre" the following temperatures were registered at Trogen, at the altitude of 892 metres, on December 6th, 1869:—At noon, $10{\cdot}2°$ C.; at 1 p.m., $9{\cdot}4°$; at 2, $-4{\cdot}6°$; at 3, $4{\cdot}3°$; at 4, $4{\cdot}1°$; at 5, $-4{\cdot}8°$; at 6, $8{\cdot}8°$; and at 7, $-4{\cdot}2°$. Thus from 5 to 6 p.m. the thermometer rose $24{\cdot}6°$ F., and fell nearly to the same extent by 7 p.m.

Dr. Joseph proposes to attack field-mice by means of a highly contagious and fatal skin-disease to which they are liable. This disease is occasioned by a parasitical fungus (*Achorion Schönleinii*). Dr. Joseph proposes to catch a few mice, treat them with the fungus, and turn them loose.

Dr. Stumpf ("Archiv. f. Klin. Medicin.") has made some interesting experiments on the influence of certain medicines upon the secretion of milk. Small doses of potassium iodide administered to a goat doubled the fatty matter and increased the sugar in her milk, the albumenoids and salts remaining unchanged. Lead slightly increased total solids and albumenoids, but diminished fatty matter. Morphine produced no perceptible change. Alcohol—whether pure, diluted with water, or in the form of beer—increased sugar and fatty matter, decreasing the specific gravity, and leaving the quantity unaffected.

Covering the skin with an air-proof varnish is, according to Dr. Ellenberger ("Deutsche Zeitschrift. Thier-Medizin") not necessarily fatal.

Prof. Kirchner, in a long series of researches, confirms the conclusion of Darwin that only the point of the root of plants is directly sensitive to gravitation.

According to H. J. Brautlecht ("Naturforscher") microbia may be found in abundance in the moisture condensing on the sides of a bell-glass placed over sand, gravel, or garden-mould which had been previously watered with a fluid containing bacteria.

Baron Ettingshausen finds that the Tertiary flora of Australia is far more nearly allied to the Tertiary floras of the other continents than to the living flora of Australia.

Mr. D. Mackintosh ("Geological Magazine") agrees with the late T. Belt in considering that only a few thousand years have elapsed since the glacial beds were laid down, thus rendering it less remarkable that no new species of animals seem to have appeared since the Glacial Age.

M. Richet has been elected to the vacancy in the French Academy of Science (Section of Medicine and Surgery) by 32 votes as against 23 polled by M. Brown-Sequard.

M. P. Mégnin ("Comptes Rendus,") gives an instance of the direct reproduction of a tape-worm (*Tænia serrata*), in the intestines of a young dog, without any possible larval migration.

Dr. C. A. Cameron, at a meeting of the Academy of Medicine in Ireland, read a paper showing that consanguineous mariages were a cause of deaf-mutism. Other medical gentlemen present gave instances of the racial injury occasioned by such marriages.

F. H. Storer ("Science") mentions that a hawk (*Buteo Pennsylvanicus*) was found, when shot, to have its stomach entirely full of caterpillars.

S. Kneeland writes to the same journal to say that the housefly is extremely rare in the Philippines.

According to the Halifax "Guardian" that town has been the scene of an Anti-Vaccination riot.

P. P. Dehérain ("Annales Agronomiques") finds that during germination young plants absorb a considerable quantity of mineral matter. Calcium ulmate seems to have in this respect a particularly favourable influence.

Dr. H. Winnacker ("Naturforscher") has made a particular study of the vegetation of sewers and of drainage channels. He finds that the Algæ which are harmless flourish best in channels which are constantly traversed by clean water. On the other hand, the Schizomycetes (including *Micrococcus*, *Bacillus*, *Spirillum*, and *Bacterium*) which are dangerous flourish in watercourses which are alternately wet and dry. A green deposit is a favourable sign.

For the normal eye every point of the spectrum appears with a different colour. For the colour-blind there are certain points in which they obtain the sensation of whiteness, because at these points the two colour-sensations which in them exclusively predominate act with equal intensity, and produce the sensation of whiteness. Herr A. König ("Verhand. Berlin Physik Gesells.") has determined this neutral point in a number of cases by means of a special apparatus. He considers that the cause of colour-blindness lies in a more or less complete coincidence of two of the three curves of primary sensibility.

M. Aug. Charpentier ("Comptes Rendus") considers that from the point of view of perception we must recognise two very distinct groups in the series of colours. The *warm* colours are distinguished from a colourless ground more easily than white, whilst the *cold* colours are less readily distinguished than white.

Professor Jäger, the "soul-smeller" (*seelen-riecher*) as he is familiarly called in Germany, delivered lately a lecture on the necessity of wearing animal fibres alone next to the skin, rejecting cotton and linen even for the linings of garments. Many passages of the lecture are said to have occasioned general cheerfulness.

Professor J. W. Spencer ("Kansas Review of Science") maintains that in the earlier portion of the Quaternary Age, the Mississippi was very much larger than at present, having in the lower part of its course an average width of fifty miles, and received the drainage of Lakes Superior, Michigan, and Winnipeg.

We observe that "right-handedness" extends very far along the animal series. Parrots hold their food by preference in the right foot, and, though we cannot speak positively, wasps, beetles, and spiders seem to use the right anterior foot most commonly.

To our non-professional mind the projected establishment of a new hospital for North London seems a step of doubtful wisdom in face of the financial difficulties with which the existing medical charities have to struggle.

M. V. Babes ("Comptes Rendus") has made a comparative study of the bacilli of leprosy and of tuberculosis, and finds that the distinctions indicated by Dr. Koch do not exist, but he points out, in their place, other differences.

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